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OF THE

ROYAL SOCIETY OF LONDON.

From November 18, 1886, *to December* 16, 1886.

VOL. XLI.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

"On Underground Temperatures; with Observations on the Conductivity of Rocks; on the Thermal Effects of Saturation and Imbibition; and on a Special Source of Heat in Mountain Ranges."

By JOSEPH PRESTWICH, M.A., F.R.S., F.G.S., &c.

Received January 24. Read February 12, 1885.

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1. *Introduction.*

The opinions of physicists and geologists as to what may be the probable thickness of the crust of the Earth differ very materially. On the strength of its great rigidity and the absence of tides, physicists contend for a maximum thickness and the comparative solidity of the whole mass of the globe. On the evidence of volcanic action, the crumpling and folding of the strata in mountain ranges, its general flexibility down to the most recent geological times, and the rate of increase of temperature in descending beneath the surface, geologists contend for a crust of minimum thickness (although respecting the measure of this there is great difference of opinion) and a yielding substratum, as alone compatible with these phenomena.

My intention here is not to enter upon the general question, but to lay before the Society the results of an inquiry on one section of it —namely, the rate of increase of temperature beneath the surface,—a subject equally affecting the argument on both sides. My attention was more specially directed to this subject in connexion with an inquiry on the cause of volcanic action, during which I found that the recorded observations gave so wide a choice in the selection of a mean rate for the increase of temperature, or as termed by Professor Everett, the "thermometric gradient," that very different values might

be and were attached to it by different writers. I was thereby led to collect the scattered evidence bearing on the subject, with a view to see whether it were not possible to fix upon some more definite and less elastic rate.

If the inquiry does not lead to any material alteration of the averages now usually accepted, it may, at all events, serve to eliminate some sources of error, and to restrict the limits within which the true rate may lie. It may also serve to show how far the differences in the observations in different geological areas are due to causes common to all, and how far they are special to each or due primarily to geological structure and local causes.

At present, owing to the wide differences in the recorded observations, it is very usual to take a general mean rate of 1° F. for every 50 to 60* feet of depth, or of 1° C. for every 30 mètres. Others take even a lesser rate of increase, and everyone must have felt the want of the greater certainty which can only be given by a restriction of the limits of variation to more definite bounds. I had already in connexion with work on artesian wells, and as a member of the Royal Coal Commission of 1866, in connexion with the "Possible Depth of Working," got together a considerable number of observations, and to those I have added a considerable number of others, many of them not before recorded. Although some of the earlier observations may seem to be of little value, I have thought it best to keep the record of all in the general list (Table I) for reference in case of need, as with corrected data respecting the surface temperature and height, some of them may possibly hereafter prove available. A list of all local observations may be useful also at any time for further research which is still much needed, particularly in connexion with artesian wells, which under all circumstances, appear to afford the best and surest index of underground temperatures.

I need scarcely say that I treat the subject solely from the geological point of view. For its physical and mathematical aspects, the papers of Sir William Thomson,† Professor Everett,‡ Professor Lebour,§ and the Rev. O. Fisher,‖ should be consulted.

* The more rapid gradient of 1° in 50 feet has been the one adopted by some physicists and geologists, but generally one of 60 feet to the degree, or even more, is adopted.

† "On the Reduction of Observations of Underground Temperature," "Trans. Roy. Soc. Edin.," (1860) vol. xxii, p. 405; "On the Secular Cooling of the Earth," *ibid.* (1862), vol. xxiii, p. 157.

‡ "On a Method of Reducing Observations of Underground Temperature," "Trans. Roy. Soc. Edin." (1860), vol. xxii, p. 429; "On Underground Temperature," "Proc. Belfast Nat. Hist. Soc. for 1873–4"; Reports of Committee of Brit. Assoc. for 1868–1884.

§ "On the Present State of our Knowledge of Underground Temperature," "Trans. North of England Inst. Min. and Mechan. Eng.," vol. xxxi (1882), pp. 59, 204.

‖ "Physics of the Earth's Crust," Chapter I.

The subject of underground temperature is far from being new. It attracted attention a century and a-half ago, some observations having been made by Gensanne in 1740 in the mines of Alsace, which if we take the mean annual temperature at the mines of 47° F., give, curiously enough, an increase of 50 feet per degree Fahr.

Towards the end of the century, a few experiments were made by Saussure in Switzerland, and by Humboldt in America, but the more important ones were those made by Daubuisson in 1803, in the mines of Saxony and France, which gave a rate of increase varying from 54 to 72 feet per degree F. De Trebra carried on similar observations in one of the Saxony mines for two years, which, taking the surface temperature at 44°, show an increase of 57 feet per 1° Fahr.

Passing over some minor observations, we come to the series of careful and systematic observations commenced in Cornwall by Dr. Forbes and by Mr. R. Were Fox, about 1820, and which were carried on by the latter continuously until 1857. Other Cornish geologists followed, Mr. W. J. Henwood especially, who between 1837 and 1858, made a large number of experiments, not only in Cornwall, but also in the mines of South America. Notwithstanding that the instruments had not the perfection of those of later date, the observations are of great value, as they were conducted under very varied conditions, and with a full understanding of the various causes of interference to be guarded against. They show very considerable variations in the rate of increase with depth. Mr. Fox records a range for 1° F. of from 32 to 70 feet, from which De la Beche deduced a mean from 46 to 51 feet; and Henwood, while finding the results to range as widely as did Mr. Fox, nevertheless estimated from a large number of averages the gradient for 1° F. to be from 37 to 41 feet only.

In 1822, Cordier published his celebrated "Essai sur la Température de l'Intérieur de la Terre," and in this he recorded the observations previously made by Gensanne, Daubuisson and Fox, together with some observations made by himself in the coal-pits of France. The several results showed variations extending from 13 to 57 mètres per 1° C., but he concluded the mean to be about 1° C. per 25 mètres of depth.

Amongst the most careful of the early observations are those made by De la Rive and Marcet in an artesian well at Pregny near Geneva. In this case, the thermometer was protected against pressure, and the result gave a rate of increase of $48\frac{1}{2}$ feet per 1° F.

This was shortly followed by Professor Phillip's well-known observations in the deep coal-pit of Monkwearmouth, Sunderland; great precautions were taken against error, and in the result an increase of 1° for every 62 feet of depth was obtained.

At this time a number of deep artesian wells were being made in France by Messrs. Degousée and Laurent and other engineers, and M. Walferdin invented an overflow maximum thermometer, which carefully guarded against pressure, gave excellent results, and which he employed to check many of the earlier observations made without this necessary precaution. The numerous artesian wells also made about this time in Algeria, Venice, and other parts of the Continent by Degousée and Laurent, gave important results. Those in Algeria seem to show that the rate of increase is there more rapid than in Europe.

Admirable discussions of the question were made from time to time by Arago: the last being that published in his "Œuvres Complets" in 1856. In this he reviews the whole subject, gives elaborate particulars of most of the recorded observations, with the advantage of more accurate surface temperatures, and concludes that the rate of increase is very variable, and may be taken at from 20 to 30 mètres for each degree centigrade.

In 1861 Sir W. Fairbairn gave an account of the observations, so frequently referred to, in the Dukinfield Colliery, where the rate of increase was estimated at 1° F. for every 79 feet.

Professor Hull also brought together in his "Coal Fields of Great Britain" (Edit. 1873), a number of observations made in the coal-pits of this country, and drew especial notice to the Rosebridge Colliery temperatures. The general conclusion at which he arrived was that the underground temperature varies, being much influenced by the dip of the strata, but may, as a mean, be taken at 60 feet per 1°.

The Royal Coal Commission of 1866–70 was the means of obtaining through its Committee "On the Possible Depth of Working," a mass of most valuable evidence from practical coal-owners, inspectors, and managers, relating to the temperatures at depths in the coal mines of Great Britain. The general opinion of these gentlemen was that the temperature increased at the rate of about 60 to 63 feet for each degree F., and the Committee concluded that the rate of increase of the temperature of the strata in the coal districts of England is in general about 1° F. for every 60 feet of depth.

In 1867 a Committee of the British Association was appointed (at the suggestion, I believe, of Sir W. Thomson) "for the purpose of investigating the Increase of Underground Temperatures downwards in various Localities of Dry Land and under Water," with Professor Everett as secretary. To this Committee we are indebted for a series of sixteen very valuable annual reports, in which are recorded a large number of observations carried on with corrected instruments supplied by the Committee, and under the able superintendence of its secretary. Each set of observations is discussed at length; the causes of interference are considered; and detailed particulars are given of all the

conditions of depth, strata, isolation, &c., under which they were made.

In the summary of results given in the Report for 1882, Professor Everett, after describing the instruments used, the methods of observation, and the many questions affecting the value of the observations, such as the heat generated by the boring tools, or by chemical action, the effects of ventilation, the convection of heat by air and water currents, &c., proceeds to a comparison of results. Taking the list of 36 localities of which Professor Everett gives the recorded results, and classifying them as—1. Metallic Mines; 2. Coal Mines; 3. Wells and Wet Borings; 4 Tunnels; he found that in the—

Metallic Mines, the increase of temperature with depth varied from 1° F. in 47 feet to 1° in 126 feet;

Coal mines, the range was 1° F. in 45 feet to 1° in 79 feet;

Wells and borings gave from 1° F. in 41 feet to 1° in 130 feet, or excluding the wells of La Chapelle and Bootle, which are open to suspicion of convection currents, the least rate of increase was 1° in 69 feet;

Tunnels: In the only two great tunnels in which observations have been made, that of Mont Cenis gave an increase of 1° for 79 feet, and that of St. Gothard of 1° for 84 feet, but in a subsequent report Dr. Stapff has shown that an important modification is necessary for a portion of the tunnel.

In deducing a mean from these various results, Professor Everett has considered it best to operate not upon the number of feet per degree, but upon its reciprocal—the increase of temperature per foot; and assigning to the results of the observations at the thirty-six localities of which he gives the list, weights proportional to the depths, he finds the mean increase of temperature per foot to be 0·1563, or about $\frac{1}{64}$ of a degree per foot—that is, 1° F. in 64 feet.* In the subsequent report of 1883, however, taking the corrected readings of Stapff for the St. Gothard and Mont Cenis tunnels, this mean is reduced to 1° F. in 60 feet.

Scattered observations have also been made within the last few years in different parts of the world, some in particular of much interest on the deep hot artesian well of Buda-Pesth, by Professor Szabo, and by others in America and elsewhere; while temperatures of a number of ordinary wells have been taken by the Rivers Pollution Commission. Amongst those to whom I am indebted for furnishing me with observations in wells and mines in their respective districts,

* Professor Everett observes, however, in a previous paper (1874) that some of the best observations give a rate of about 1° F. for 56 feet of descent, and that this seems a fair average.

not hitherto recorded, are Professor G. Dewalque of Liège, Professor Gosselet of Lille, M. F. L. Cornet of Mons, and others.

Professor Lebour has further discussed the subject, with reference especially to the nature of the experiments still required to improve our knowledge of it, in an interesting paper read before the North of England Institute of Mining and Mechanical Engineers, in 1882. In this he gives a list of observations made at fifty-seven localities, and deduces from them an average rate of increase of 1° F. per 64·28 feet of descent; but he remarks that all the observations recorded are by no means of equal value, and points out that the mean rates of increase in the best observations cluster about the numbers 1° F. per 50 or 60 feet.

General List. (*Table I, p.* 56.)

I have given in a general list all the observations on underground temperatures of which I have been able to find a record.* The number of different localities, mines, &c., where such observations have been recorded, amounts to 248, and the number of stations† to 530. I have only omitted a few which were obviously wrong, together with some of the observations repeated frequently in the same mine, or repeated more frequently than necessary for our object in mines in the same district, and which would only add needlessly to the length of the list.‡ Neither have I considered it necessary to give in all cases the full details of the observations, as these will be found in the original papers referred to. I have confined myself to giving the principal and essential facts and results.

The observations are given in the order of date, an order which, as a general rule, agrees with that of their reliability, although there are several remarkable exceptions. The superiority of the later observations consists chiefly in the perfection of the instruments and methods of experimentation. Most of the more general disturbing causes interfering with accuracy of the results were well understood and guarded against by the early observers.

The particulars of depth, temperature, and modes of proceeding, are given in the terms of the original observers. On two points only, on which their information was often unavoidably imperfect, have I made any alteration—points which even now are in many cases not accurately determined—viz., the mean annual temperature of the surface, and the height of the surface above the sea-level. These

* I should feel greatly obliged by information respecting any other observations.

† By "station" I mean all the separate points in a mine, or at the several depths in a well, at which the observations have been made.

‡ Mr. Henwood's long and valuable series of observations in the Cornish and foreign mines will be found in vols. v and viii, "Trans. Roy. Geol. Soc., Cornwall." I have given all his principal instances.

alterations will account for occasional differences in the thermometric gradients, between the observations as originally recorded and as given in these lists, which in most cases will be found referable to my taking a different mean annual temperature to that used by the original observer.

HEIGHT OF THE SURFACE.—As this influences both the surface and underground temperatures, it is important that it should be determined with precision. This is not always possible, although the later publications of the Ordnance Survey have in many cases furnished us with levels, which, if not at the precise spot, are sufficiently near to form, with a knowledge of the country, a near estimate; but not unfrequently mines are in out-of-the-way places, where only roughly approximate estimates can be made. Several of the surface heights given in this table necessarily come into this category, and are therefore subject to future correction; while there are many cases in which I am unable to give even an approximate height.

MEAN ANNUAL TEMPERATURE.—This is a point on which it is indispensable to have exact information, for it is one in which the difference even of one or two degrees materially affects the calculations. There are yet, however, many of the places in the lists, where satisfactory information is wanting, and the accepted temperature may therefore require revision. For places on the Continent, the works of Arago,* which embody all that was then known of the mean temperature of places, both in France and other parts of the world, are of great value. The publications of the Meteorological Society of Scotland have done much to give us exact data respecting the mean temperature of places in Great Britain,† and I am further indebted to Mr. Robert H. Scott, of the London Meteorological Office, for valuable information respecting the latest recorded mean temperature of places both in this country and on the Continent. There are, besides these, the older and more general tables of Dove.

Where the temperature of the locality cannot be had, that of some place near and on or near the same level has been taken. Where there is an important difference of height between the place at which the surface temperature has been taken and the site of the underground experiment, an approximate allowance of one degree F. (+ or −) is made for every 300 feet of difference of level. When, however, as is sometimes the case, a mean annual surface temperature is not obtainable, a near approximation may often be made by means of ordinary surface wells of about 40 to 50 feet deep, and springs. This plan has been, especially in the earlier observations, frequently adopted.

What is really desirable is that the datum line whence to commence

* "Notices Scientifiques," vol. v, p. 518. Edit. 1857.

† Especially the number of Journal for November, 1883.

the measure of the increase of temperature with depth, should be placed at that point below the surface, at which the changes of annual temperature cease to have effect. Any such determination* might also with advantage be applied to the correction of the past observations.

In the absence of this information, we have for the present to be satisfied with the mean surface temperature, where known, and take that as the datum line from which to calculate the descending rate. Sometimes this has been the mode adopted, and the gradient calculated from the surface; at other times it has been calculated by supposing the mean invariable surface-temperature to lie at a depth of 50 feet, and taking that as the datum level from which to start. Thus, some of the original estimates have been founded on calculations commencing from the surface, and others at 50 feet beneath it.

Either, therefore, some of these estimates are too high in consequence of the deduction of 50 feet from the depth, or others are too low in consequence of not making an allowance for the zone of variable temperature. But as the mean annual temperature of the ground at the surface generally in these latitudes exceeds that of the air by about 1°, it follows that if we take the mean temperature of the air at the place of observation, and calculate the rate of increase from the surface, instead of allowing 1° for the higher temperature of the ground and with a datum line 50 feet lower, we shall come to nearly the same result. At the same time it must be admitted that in so doing, we may sometimes be liable, especially with stations of moderate depth, to make the rate of increase with depth less than it should be. It is therefore the more important in future observations to determine if possible the temperature at a depth of 50 to 60 feet—or where the first bar of uniform temperature may happen to be—and to calculate from that point the rate of increase of temperature with depth. Failing this, starting from the surface with the mean annual temperature of the place will no doubt give an approximate result. Thus the mean temperature of Paris is 51° F., and the temperature of the cellars of the Observatory at a depth of 95 feet is 53° F., or 2° higher, so that if in this instance, whether we start with a surface temperature of 51° or a temperature of 52° at a depth of 50 feet, the result would be almost exactly the same.

In looking over Table I, the essential differences in the results obtained in Mines, Coal-pits, and Artesian wells will be at once apparent. This arises both from the circumstance that not only in each of these

* This was done in the case of the experiments at Grenelle, where a datum line based on the temperature and depth of the cellars of the Paris Observatory was employed. These are 29 mètres (95 feet) deep, and the thermometer stands invariably at 11·7° C. (53° F.), the mean annual temperature of Paris being 10·6° C. (51° F.). This is, however, a greater depth than necessary.

are the geological conditions very dissimilar, but also that the disturbing causes are of a different order. In Mines, the latter are attributable to—

1st. Air currents established for ventilation, and by convection;
2nd. The circulation of underground waters;
3rd. Chemical reactions;
4th. The working operations.

And in Artesian Wells, to—

1st. The pressure of the column of water on the thermometer;
2nd. Convection currents in the column of water.

While a general cause affecting each group is conductivity, which variably influences all the rocks, and which is itself liable to be influenced by a number of causes to which we shall refer separately.

We shall be able therefore to deal more readily with the subject, if, as Professor Everett has done, we divide the observations in Table I into groups in accordance with these considerations, and take each separately in the following order:—

I. Coal Mines.
II. Mines other than coal.
III. Artesian Wells and Bore-holes.
IV. Tunnels.

I. Coal Mines. (*Table II, p.* 88.)

Considering the general uniformity of geological structure of the Coal-measures, the temperature observations in coal mines are more discordant than might be expected. This depends upon various causes, of which ventilation is seemingly the principal, while in some cases differences of conductivity may possibly have influenced the results. It is true that in most cases the sources of error have been carefully guarded against, but it is doubtful whether in others sufficient allowance has been made for them, and whether in some they have not altogether been overlooked. The main disturbing causes are as follows:—

Loss of Heat through Exposed Surfaces.—To guard against the cooling of the coal or rock produced by ventilation, it has been customary to place the thermometer in holes 2 to 3 feet deep, but it is a question whether this is sufficient, for we are rarely informed, as is essential, of the precise time that the face of the coal or rock has been exposed. Even in the best observations we are only told that they have been made in "freshly exposed faces," which may or may not be sufficiently soon. For on these working faces the ventilation is necessarily well maintained, and consequently the difference between the temperature of the air and of the strata is there often most considerable, and the cooling of the rock very rapid.

On the surface of the ground the diurnal variation of temperature

extends to the depth of about 3 feet, and it has been shown by means of thermometers sunk in the grounds that at a depth of*—

1 foot	the monthly	fluctuation	is from	33°	to 54·0°	or = 21·0°
2 feet	,,	,,	,,	36	to 52·5	or = 16·5
4	,,	,,	,,	39	to 51·8	or = 12·8
8	,,	,,	,,	42	to 50·0	or = 8·0

At the depth of 29 feet at Greenwich, the annual fluctuation is still 2° to 3° F., and to reach the limits of range, or where it is at last reduced to $\frac{1}{16}$th of a degree F., we must go in this climate to a depth of from 50 to 80 feet.

Now it is evident that, owing to ventilation, there is a permanent difference between the temperature of the air in a mine and the temperature of the rock, analogous to the diurnal variation on the surface of the ground, and that its effects will extend with equal, if not greater, rapidity in the one case as in the other.

Any surface of rock exposed to air of a lower temperature will lose heat with a rapidity proportionate to the difference in temperature between the two bodies and to the conductivity of the rock. Therefore as convection currents and ventilation are constantly introducing into the shaft and mine air of a lower temperature than the underground rocks, these latter must suffer a loss of heat from the moment they are so exposed. To avoid this, the best observers have, as just mentioned, operated in freshly exposed surfaces, where the loss is at a minimum: on the other hand, numerous observations have been made on surfaces exposed for a length of time and often under conditions of which we are not informed.

Thus in the well-known case of the Dukinfield Colliery, near Staleybridge, it is stated that the bore-holes were driven to such a depth as to be unaffected by the temperature of the air in the shaft, and the thermometers were left in the holes from $\frac{1}{2}$ to 2 hours; but we are not informed of the depth of the holes, nor of the temperature of the air in the shaft, nor of how long the rock had been exposed. We know only that the shaft, which was a very large one and carried to a depth of 2151 feet, took 10 years to sink. At this depth the temperature of the rock was 75° F., and the estimated rate of increase was calculated to be 99 feet for 1° F. The observations were carried on during the whole of the time that the work proceeded. Although we do not know the temperature of the air, the action of convection currents or of ventilation is clearly shown in the irregularities of the rock temperatures in summer and winter, even at great depths. Thus, on the 12th June, 1849, the shaft had reached a depth of 704 feet, and the rock temperature was 58°. The same temperature was noted with slight fluctuations until the 22nd December, at which time the

* Prof. James D. Forbes, "Trans. Roy. Soc. Edin.," vol. xvi, p. 189.

depth had been reached of 810 feet. In February, 1857, at a depth of 1450 feet, the temperature was 67¼°, which rose with abnormal rapidity to 72¾° in August, at a depth of 1689 feet. It then slightly decreased, partly recovering and ending in October at 72¼°, when the depth was 1840 feet. In March, 1858, it had fallen to 71½° at 1881 feet, and then rose by July, at 2055 feet, to 75½°. After the winter, in March, 1859, at 2151 feet the thermometer stood at 75°, or half a degree less than it did in July at a depth of nearly 100 feet less. I can only attribute these fluctuations to the influence of the seasons and convection currents, or to some unmentioned form of ventilation. A new shaft sunk in 1858, not far from the other, gave, in the same way, analogous results, viz., an increase of 1° F. for every 90 feet.

When, however, at a subsequent period, and when the works of this colliery had been carried to the great depth of 2700 feet, observations were made in galleries at a distance from the shaft, with instruments furnished by the British Association Committee, and in holes 4 feet deep, an amended reading of 1° for every 72 feet was obtained. Still that rate of increase is slow, and Professor Everett has drawn attention to the fact that there are other collieries such as those of Ashton Moss, Denton, and Bredbury, within a few miles of Dukinfield, in which the results are in close agreement with those obtained in the latter. It has been suggested in explanation of this, that the rapid dip of the strata at Dukinfield facilitates the transmission and escape of heat. It is quite possible that this cause may have an influence, as the experiments of Herschel and Lebour have shown experimentally that the conduction of heat is more rapid along the planes of cleavage, or bedding, than across them. Nevertheless no such difference exists in other mines where the stratigraphical conditions are alike; the strata in the Liège, Mons, and Valenciennes coalfields (Nos. 140, 214–16, 145) are more disturbed, and dip at more rapid angles than at Dukinfield, and yet the deep temperatures there give no such slow rate of increase.

Again, Mr. Garside draws attention to the circumstance that the Red Sandstone, overlying the Coal-measures in the area, is highly charged with water, which is largely drawn upon for water supply. This may reduce the temperature of that mass of rock, but the effects would hardly reach to the great depths and extent observed.

Some of the anomalous results may also be attributable partly to the excellent ventilation of these deep pits. At the Dukinfield Colliery, about 58,000 cubic feet of air circulate through the pit per minute. The mean annual temperature of the air at the surface is 48° F., while the return air from the pit has a temperature of 73°. But then again the ventilation at Rosebridge and other large pits—where the rate of the increase of temperature is more rapid and nearer the probable normal—is not less carefully attended to. Still,

that ventilation has a preponderating influence is shown by an experiment of Mr. Wynne, who found that in a rise of the Dukinfield Colliery, where owing to defective ventilation the temperature of the air and rock were both 76°, the rate of increase from the surface gave only 55 feet per degree.

Other causes have also been suggested for the lower temperature of the Dukinfield pit. Mr. Dickinson states that before the shaft was sunk, two of the principal seams of coal in the upper part had been worked away from other pits, and that the lower seams had been worked a long way from the outcrop down towards the Astley pit.*

The Dukinfield observations, therefore, while they confirm the general law of increase of temperature with the depth, are scarcely admissible in estimates concerning the exact rate of increase of temperature with the depth, and it would be safer to omit them in such calculations.

I do not mean to imply that good observers have or would select long exposed and cooled surfaces, but others have done so, and my object here is to show that cooling must at once commence, and that it is difficult to escape in any part of the pit, or however early the experiments are made, from the effects of ventilation. It is not a question of weeks or months, but of days, if not hours.

Effects of Ventilation.—The evidence given before the Committee of the Royal Coal Commission of 1867, "On the Possible Depths of Working," is particularly valuable for the information it affords respecting the effects of ventilation.

At the Pendleton Colliery (Nos. 104 and 122), near Manchester, the coal in a level 500 yards from the down brow, had a temperature of 70°, and at 1000 yards distant, in the same level, and when just opened, it had a temperature of 83° F.

At the Rosebridge Colliery, Wigan, the temperature of the Raven coal in the shaft was 78°, whilst at 630 yards distant it had a temperature of 88°. The air at the bottom of the down-shaft at a depth of 1800 feet was 60·5°, and, after circulating 1500 yards, 73°. At another time it was 51·5°; after circulating 2400 yards, 67·5°; and after 3140 yards, 71°. In July, 1858, the strata at the same depth had a temperature of 80°; in March, 1861, it was reduced to 72°.

At the Annesley Colliery (No. 106), Nottinghamshire, a fresh cut coal at a depth of 1425 feet had a temperature of 73°, and six months later it had fallen to 64°. In another case, the coal cooled 11° in three months.

The volume and velocity of the air currents vary greatly. In one place a circulation of 5000 cubic feet per minute is recorded. In another pit the circulation was 100,000 cubic feet per minute, while

* Coal Commission Report, vol. ii, "Depth of Working."

in the upcast shaft, 7½ feet in diameter, of another pit, 150,000 cubic feet of air passed per minute.

At the Hetton Colliery (No. 116), Durham, the dependence of the temperature of the strata upon distance from the shaft, in connexion with ventilation, is well shown in a table by Mr. Wood, of which the following is an abstract.

Surface temperature at date = 36°. Holes in coal 3 feet deep.

Depth from surface.	Distance from shaft.	Cubic feet of air per minute.	Temp. of coal.	Temp. in current of air.	Rate of increase in depth for 1° F.
1100 feet	312 yards	104,000	60°	50°	100 feet
1270 „	915 „	50,000	63	58½	91 „
1360 „	1640 „	18,600	66	62	80 „
1354 „	3664 „	3,200	68	68½	67 „
1400 „	3550 „	4,972	70½	72	64 „
1395 „	4332 „	5,000	71	73	63½ „

In this case we have four observations made nearly at the same depth, and yet showing a rate of increase with depth varying from 80 feet to 63½ feet for 1° Fahr.

In the Seaham (Durham) Colliery the circulation of air is very large,—193,346 cubic feet per minute. At a depth of 1524 feet and 1642 yards distant from shaft, the temperature of the air was found to be 61°: at 2726 yards distant and at the same depth it was 67°; and at the face of the coal 82°. The temperature of the return air=76½°.

At the Eppleton Colliery, Durham, in October (?) the temperature of the air at the intake at a depth of 850 feet and 110 yards distant from shaft was 44°, and after circulating 1960 yards, 59½°, or an increase of 15½°.

Some joint observations by Mr. Wood and Mr. Dickinson in the Monkwearmouth Colliery (No. 30), Sunderland, gave the following results. 29th March, 1870. Temperature of surface = 42°.

Depth.	Distance from shaft.	Temp. of the air.
1676 feet	Bottom of down shaft	50° F.
1676 „	2134 yards	66 „
1638 „	2850 „	74 „
1646 „	3112 „	81 „
1640 „	4030 „	82 „

This pit is so dry that it requires water to lay the dust.

Mr. Dickinson found that at a depth of 2088 feet in one of the Pendleton pits the temperature at 500 yards from the engine brow was 78°, and at 1000 yards distant 82°. In the same pit at a depth of 2214 feet and distant 200 yards from the engine brow, the coal in

a hole 3 feet deep was 76°, and at a distance of 930 yards 82°, while in a hole 7½ feet deep the temperature was 84°. The general opinion amongst the witnesses seemed to be that the effect of ventilation was to reduce the temperature of the pits some 9° to 10° F., though some estimated it at as much as 15° to 20°.

The effect of exposure is well shown in the following records from one of the pits at Ruabon.

Temp. of air at date on surface.	Depth.	Temperature of air in gallery.	Temperature in holes 3 feet deep of coal.	Temperature in holes 3 feet deep of bed below coal.
60°	1002 feet	58·0°	60·0°	67°
62	1503 ,,	58·5	70·5	68
57	1605 ,,	71·0	73·0	77
65	1770 ,,	71·0	78·0	74

No other particulars are given. The rate of increase here for the first 1000 feet is 1° for every 95 feet, whilst for the whole 1770 feet the rate of increase is 1° for every 61 feet. This is clearly due to the circumstance that the shaft had long been sunk to 1004 feet and the mine cooled, and only recently opened to 1770 feet.

Mr. Lupton mentions that in a pit 1425 feet deep, the fresh cut coal had a temperature of 73°, whereas at the end of six months it had fallen to 64°. In another case a period of three months had sufficed to reduce the temperature 11°.

After the air in the pits has circulated a certain distance (estimated by one of the witnesses at one mile), it may become in the face of the workings hotter than the rock; while the difference, which arises from the heat of the lights and the men in an instance given, does not exceed +2°.

The extent to which the temperature of the air affects that of the coal is also shown in the table of experiments, given on next page, made by Mr. Wood in another Durham Colliery (No. Report, p. 140). Here the difference between the coal and the air in the part of the pit through which the intake air circulates, is from 4° to 6½°, or an average of 5°; whereas in the return air at the same distances from the shaft, the differences are reduced to an average of 1¼°.

In the Crumbouke mine the face of the coal which had been worked two years had, according to Mr. Dickinson, lowered 4°. A similar loss was shown at the Rosebridge Colliery, but after a longer interval. In an adjoining pit, the difference between the temperature of the air in the galleries and of the coal varied from 10° to 12½° F.

At the Rosebridge Colliery other observations made three, four, and nine years after opening the colliery showed, that at 1410 feet deep, and 200 yards from shaft, the temperature of the coal was = 66° F., and at 360 yards = 70° F., while at 1800 feet deep and 215 yards

Jane Pit, Hetton Colliery. Depth below surface.	Circulation of air per minute.	Distance from shaft.	Temperature of				Temperature (the mean annual surface being 47°) calculating	
			Intake air.	Coal.	Return air.	Coal.	1° for 60 feet of depth.	1° for 50 feet of depth.
feet.	cub. ft.	yds.						
1080	41,800	410	58½°	65°	70½°	68°	65°	68½°
1270	41,580	955	59½	65	71	73½	68	72½
1320	30,030	1247	62	66½	69½	72	69	73½
1360	28,000	1640	63	67	70	72½	70	74¼
Average 1357½	35,352	1063	60¾	65¾	70¼	71½	67½	72¼

from shaft the temperature of coal was = 65° F. Mr. Bryham states that the reduction of temperature effected in this pit by ventilation amounts to from 15° to 20° F., or even more in a strong current.

Other returns give the column of air circulating through the pits at from 50 to 150 million cubic feet in the twenty-four hours, with a gain of temperature of from 10° to 20° F., which shows how great the abstraction of heat from large and well-ventilated coal-pits must be. These effects are rendered very apparent by some tables put in by Mr. Lindsay Wood, of which the following is an abstract:—

	Depth below surface.	Distance from downcast shaft.	Intake air, cubic feet per minute.	Temperature of	
				Intake air.	Return air.
	feet.	yards.		Fahr.	
Eppleton Colliery, Durham,	1100	312	78,720	61·0°	70°
13th Oct., 1869. Mean surface temp. = 53° F.	1450	2925	11,550	69·0	
	1395	4130	4,500	72·0	
Working face	„	4440	„	74·0	
The same pit *off work*, 23rd	1100	312	52,200	57·5	69
Oct., 1869. Surface temp. = 52° F.	1395	4130	3,240	71·5	
Monkwearmouth Colliery,	1676	80	38,250	56·5	77
25th Oct., 1869. Surface temp. = 45° F.	1646	1430	71,500	63·0	
	1638	2850	5,000	75·7	
Working face.....	1646	3256	„	81·2	
Murton Colliery—*off work.*	1339	410	76,160	54·0	68
Working face.....	1374	4400	1,600	70·0	
Surface temp. = 50°.					

Even in the two pits off work, we have in the one instance 44,550 cubic feet of air passing out per minute, with a gain in temperature above that of the outer air of 17° F., and in the other case 25,500 cubic feet pass out with a gain of 18° F.

In these instances the temperature of the outer air was only in one observation under 50° F. At other seasons of the year, with the outer thermometer lower, and with the difference of temperature between the circulating air and the strata greater, the loss of heat would proceed still more rapidly. Mr. Forster mentions that he has known ice form at the bottom of a coal shaft, and Mr. Bald, speaking of the Whitehaven Collieries, states that the circulation of air sometimes causes the water to freeze on the sides of the shaft, and "even form icicles upon the roof of the coal within the mine," whilst the air from the rise-pit issues in a dense misty cloud.

In consequence of the allowance necessary to be made for these effects of ventilation, some of the witnesses, while taking the actual rate of increase at about from 50 to 60 feet for each degree F., considered that the normal rate might not be more than 50 feet per degree.

As a rule, the deeper the mine the greater must be the ventilation, and therefore, the more rapid must be the cooling of the underground strata. But the amount of ventilation depends also not only on depth, but on the quantity of gas present in the coal. Mr. Warington Smyth says,* that "in round numbers 100 cubic feet of air per minute may be required for the health and comfort of each person underground, or for 100 men, 10,000 cubic feet; but if fire-damp be given off—say at the rate of 200 cubic feet per minute—we should need at the very least thirty times that amount of fresh air to dilute it, or 6,000 cubic feet in addition. Increase the number of men and liability to gas, and 40,000 to 60,000 cubic feet of air may be indispensable for safety."

It is evident, therefore, that the rate of cooling from ventilation in different coal mines and at different depths is likely to be very variable. The greater the heat and the more gas in the coal, the more rapid will be the cooling, and this may possibly account for the great discrepancies between the thermometric gradients at different collieries. Until, however, we are in possession of all the collateral conditions, it will not be possible to assign in each instance, the precise weight to be attached to this disturbing cause.†

Other Causes which may affect the Temperature of the Coal Strata in Underground Workings.—Apart from the chemical decomposition of

* "Coal and Coal Mining," p. 205.

† Mr. W. Warington Smyth gives, in his Anniversary Address to the Geological Society in 1868 (pp. 79—87), some interesting particulars respecting ventilation and other causes affecting underground temperatures.

minerals, which is of rare occurrence in coal mines, and the heat arising from the men, horses, and lights, which can only be of importance in shallow mines, for in deep mines it is rarely that that source of heat brings up the temperature of the air to the normal temperature of the rock, there are few causes likely to produce an abnormal rise of temperature. There is one, however, which though of exceptional occurrence, should be noticed in connexion with this subject, and that is the heat produced by the crushing of the rocks which sometimes takes place in coal mines.

The Crushing of Rock.—It is certain that in coal mines where pillars are crushed and the strata pressed up in *creeps*, more or less heat is liberated. No experiments, however, have been made to determine what, in these cases, are the exact effects produced, but some of the evidence given before the Coal Commission fully establishes the general fact. Mr. Elliot,* in speaking of one of the pits in South Wales, mentioned that when "a creep takes place, he has known the temperature very much increased," and in one case where "the pressure began to crush the pillars, the heat produced was so great that he feared it would set fire to the coal." In some cases the pressure has been such as "to grind the rock to powder, like the effect of a dozen mill-hoppers, and this was accompanied by considerable heat." He had often found the air very hot when a sort of temporary creep occurred.

Escape of Gas.—On the other hand, a cause productive of a loss of heat is a more constant disturbing cause. There are few coals which do not give off gas when first exposed. In some seams it may be observed exuding from the freshly broken surfaces with a hissing sound; and, if in large quantity, as in the case of the so-called "blowers," or sometimes near faults, it issues with a rushing noise like the steam from a high-pressure boiler. Some of these blowers will be exhausted in a few minutes, others will last for years—like that at Wallsend, which gave off 120 feet of gas per minute.† The common gas on these occasions is light carburetted hydrogen. It must exist in the coal under the enormous pressure either highly condensed, if not in a liquid state, otherwise it is hardly conceivable how the discharge could be maintained so long. The pressure of this gas is said to equal sometimes 300 to 400 lbs. to the square inch.‡

In any case the escape of this gas from the coal, in which it apparently exists in an infinite number of small cavities, must be to

* Coal Commission Report "On Possible Depths of Working," p. 112.

† "Coal and Coal Mining," p. 204.

‡ [Sir Frederick Abel states that, if cavities are bored into the coal and plugged, the gas speedily accumulates so as to exercise a pressure of several hundred pounds upon the square inch, as indicated by pressure-gauges fixed into the cavities. "Nature," December 3rd, 1885. Under this enormous pressure we do not know what the critical point may be.—January, 1886.]

reduce the temperature of the body from which it escapes, though here again no special experiments have been made to ascertain the exact loss of heat from this cause. I find, however, one of the witnesses* on the Coal Commission remarking that in a pit the depth of which had been increased from 830 to 1588 feet, the temperature of the coal was lower at the greater than at the lesser depth, and he attributed this to a strong blower of gas issuing from the coal, which at that point was sensibly cooler to the touch.

On a Table also put in by Mr. Elliot, and referring apparently to the same case, it is stated that,—

In the Lower Duffryn Mine at a depth of.....	1588 feet,
—and distant from shaft	1850 yards,

the temperature of coal in a wet hole with a blower of gas was 62° F., whereas it was found that—

At a depth of.....	1269 feet,
Distance from shaft	1020 yards,

the temperature in a dry hole with little gas was 74° F.

At the Cym Neol Colliery, at depths of 990 and 1150 feet, and 1350 and 1070 yards distant from shaft, the temperature was respectively 62° and 65°, which is abnormally low, but it is stated that the observations were made in wet holes *with blowers*.

This escape of gas from the body of the coal may account for the observation of another witness to the effect that "the coal gives out heat quicker than the rock," and that the temperature of the coal is, on the whole, less than that of the associated rocks. In several cases observations have been made in the same pit, both on the coal and on the underlying shale, and generally the temperature of the rock has been found to be higher than that of the coal. In the Rams Mine, Pendleton, for example, the temperature of the coal in a hole 3 feet deep was 82° F., and in the floor 83° F.; in the Crumbouke Mine the coal was 80° F., and the floor 82° F.; and in other levels it was 80° and 80°, and 78° against 82° F. In one of the Ruabon collieries the relative temperatures varied as follows:—

Depth	1002	feet;	temperature =	60°	F. in coal, and	67°	F. in floor.
„	1503	„	„	70½	„	68	„
„	1605	„	„	73	„	77	„
„	1770	„	„	78	„	74	„

As the conductivity of the coal (0·00068) is less than that of sandstone (0·00672) or shale (0·00235), the coal should retain its heat longer than the rocks. If the coal is cooler than the rock, it must

* Mr. Wilmer: "Coal Commission," p. 118.

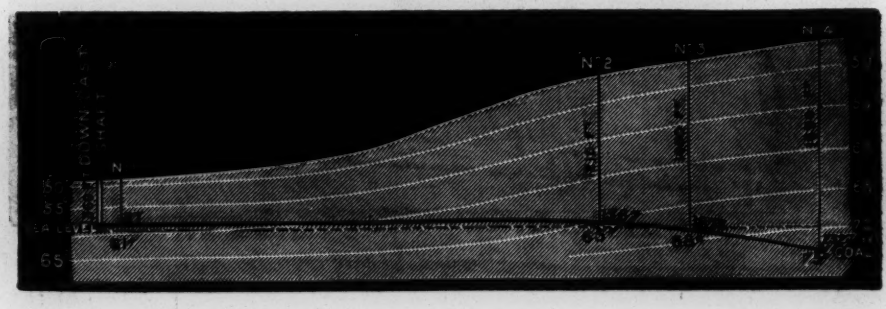
arise from an independent source of cooling in the coal, such as that just mentioned which would be caused by the escape of gas condensed under great pressure. It is an effect important to notice, inasmuch as not only is the discharge of gas from coal of frequent, and often constant occurrence, but also from the circumstance that it affects especially the fresh opened surfaces of coal in which the temperature observations have so commonly been made.

The Effects of Irregularities of the Surface.—Although it was known that the underground isothermals under mountain masses are not prolonged in horizontal planes, but follow in a certain ratio the curves of the ground above, little was known to what extent mines were affected by the smaller irregularities of the surface until Mr. George Elliot, M.P.'s experiments in coal pits in Glamorganshire, showed that there is a sensible increase of heat with an increase in the mass of the superincumbent strata. This important point has been rarely, or never sufficiently, taken into consideration in any of the underground temperature observations.

The following observations and sections of Mr. Elliot* laid before the Coal Commission were mostly made in abandoned parts of the pits, so as to avoid as far as possible both the effects of ventilation and of working, and the holes, in which the thermometers were placed, were 4 feet deep. Only the depths and temperature of the coal are given in the original sections. I am responsible for the isothermal lines which I have plotted on these data, for the purpose of showing the more precise manner in which the temperature at depths is influenced by the surface heights.

At Upper Duffryn Colliery, in South Wales, the top of the shaft is 400 feet above the sea level, and the shaft 360 feet deep. The seam of coal has been followed southward for a distance of 2327 yards, at

FIG. 1.—Section at Upper Duffryn, Aberdare District.



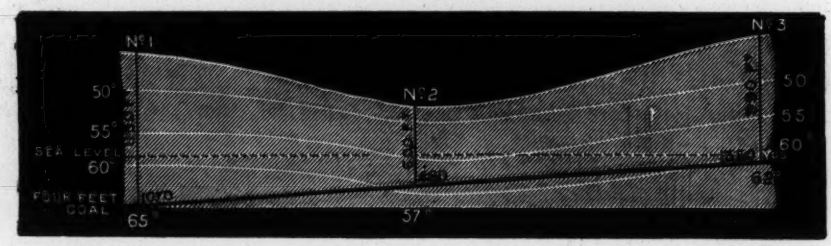
which point it is 190 feet lower in respect to the sea level than at the entrance. This difference might account for a lowering of 3° to 4° in the temperature, but not for the difference of 14°, which is due to

* "Coal Commission Reports," vol. ii, p. 105—111.

the circumstance that at this point the surface of the ground has risen so much that the seam of coal lies 1690 feet below the surface, or 1330 feet lower than at the bottom of the shaft. The distance from the shaft might partly account for this difference, but only partly for that between stations 2, 3, and 4.

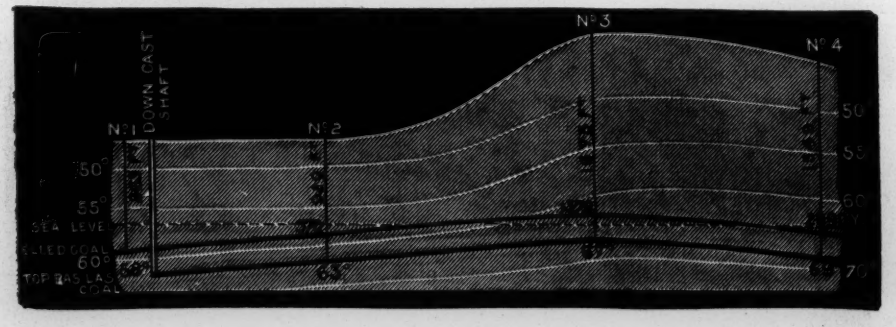
At the Cym Neol Colliery one station is under a valley, and two under the adjacent hills, rising about 500 feet above the valley. No. 2 station is only 480 yards distant from shaft (not in section), against 1070 from Station No. 1, and 1350 yards from Station No. 3. At that time the temperature of the outer air was 69° F.

Fig. 2.—Section at Cym Neol, Aberdare District.



In a similar way the New Tredegar Colliery is situated in a valley 670 feet above the sea level, and the works pass under a hill which rises 617 feet above the valley, with the effect of raising the temperature of the same coal 11° F., though 2° to 3° of this may be owing to distance from shaft. At the time the thermometer outside stood at 60° to 70° F.

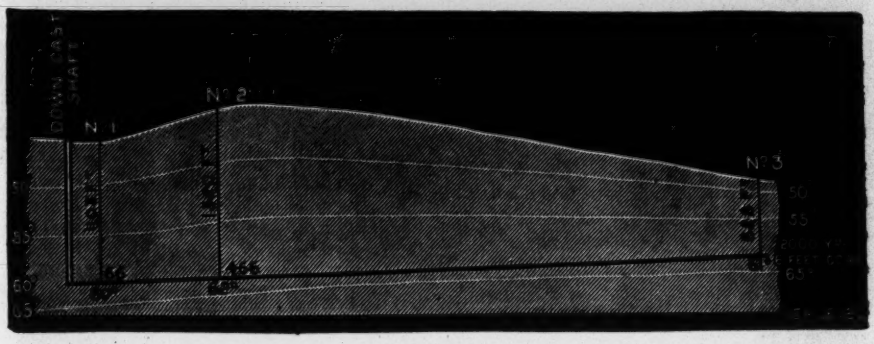
Fig. 3.—Section at New Tredegar, Aberdare District.



At the Vochriw Dowlais Colliery, on the other hand, the top of the shaft is on a hill 1330 feet above the sea level, and the seam of coal, which there lies 1103 feet below the surface, is followed first under a hill which rises 217 feet above the pit mouth, and then under a valley only 818 feet above sea level, or 512 feet lower than the pit mouth. Notwithstanding the distance of Station No. 3 from the shaft, the fall

of temperature due to the fall of the surface is apparent, though the temperature at Station No. 2 is too low, which the witness attributed to a strong current of air passing at the time. Outside the thermometer marked 59° F.

FIG. 4.—Section at Vochriw Dowlais, Aberdare District.



It will be noticed that the observations in all these pits were made when the thermometer outside stood high, so that the cooling effects of the outer air would be less at the stations near the shaft than at other seasons.

There is no other coal-field in Britain so hilly as that of South Wales; still it frequently occurs that a shaft is sunk in a valley, and the works pass under adjacent hills (or the reverse), which though not so high as those of Wales, are of sufficient height to influence the temperature more or less.

Many of these are points which have commonly not been noticed in the discussion of this question, yet they must not unfrequently have influenced the observations. In all coals there is an escape of gas when first opened out, and in some it is very large, as in several of the South Wales pits; others are merely called "gassy," and there fewer precautions are required. Amongst the pits in England, where recorded temperature observations have been made, and which are situated in districts sufficiently hilly to be affected by irregularity of surface, are the Dukinfield and some of the pits in the Manchester district.

Selection of Observations.—It will be apparent from the existence of these many disturbing causes, that not only should we know that the instruments employed are properly constructed and properly used, but we should also in all cases know,—1st. Height of the ground above the sea level. 2nd. The exact mean annual temperature of the surface. 3rd. Depth beneath the surface of the ground of each station where the observations are made. 4th. Distance of those stations from the shaft. 5th. Temperature of the air and volume in circulation at the stations. 6th. Exact length of time that the face of the coal or rock

has been exposed. 7th. Whether or not there is much gas given off from the coal. Besides these more essential points, it is well to know the temperature of the air outside, the dip of the strata, their hydro-geological conditions, together with any local causes tending to increase or lower the temperature.

It is only when all these essential conditions are known and can be taken into account, that we can hope to arrive at a more exact estimate of the real rate of increase of temperature with depth in Coal Mines. It is to be feared that very few of the observations recorded in Table II satisfy this standard. While they present many points of interest, and confirm the general fact of an increase of temperature with depth, they fail, I think, to give the more precise information required. For these reasons I feel that only a very small selection can be made, and I doubt whether those even give sufficiently true readings, though they may give the best approximation that can at present be obtained.

In making the selection, the points I have looked to are (1) accurate mean surface temperature, (2) height of ground, (3) temperature of air in gallery—or, when that is not given, such a distance from shaft as will ensure the minimum of difference between the temperature of the air and the rock*—(4) distance of the working face from the shaft, and (5) depth of trial hole. Permanence of temperature at a station is not, as it has been often considered, a sign of its correctness, or of its being the true normal temperature. On the contrary, when it is stated that the thermometer in the same hole continues to give the same reading for a long period, it is evident that, instead of having a more definite value, it cannot represent the normal temperature of the rock, but that of the rock or coal after cooling to a point at which an equilibrium has been established between the temperature of the strata and that of the circulating air. Such readings therefore are too low.

Amongst the best recorded observations are those at Boldon in the Newcastle coal-field (No. 150). The holes in the rock were there 10 feet deep, the temperature of the air in the gallery was nearly the same as that of the rock, and the mean annual temperature well known.

At the North Seaton Mine (No. 217), the station being half a mile from the shore, and more than that distance from the shaft, a certain uniformity of temperature between the rock and the air was necessarily ensured, although that is not mentioned.

The experiments at Hetton Colliery (No. 116) seem to fulfil the required conditions, but I do not place implicit reliance on them, because the holes were first filled with water and then left for forty-eight

* It is said that at about the distance of a mile from the shaft the temperature of the air approximates to that of the coal, but this of course is a very variable result.

hours, after which the thermometers were left in them for twenty-four hours, so that the coal was exposed for at least for three days, if not more, to a current of 5000 cubic feet of cooler air per minute. Further, it is stated that there was no difference in the temperature after the lapse of a fortnight. I infer from this that the coal here again had reached a cooling point at which the equilibrium between the air and coal had been temporarily established.

The experiments at South Hetton (No. 136) have especial value, as they were made in a bore-hole drilled into the strata at the bottom of a shaft, 1066 feet deep, to a further depth of 878 feet. The first reading made on the completion of the hole, and while yet dry, gave a temperature of 96° F. Three years later the experiments were repeated, but in the meantime the hole had become filled with water, and silted up to the depth of 234 feet. The thermometer was, however, pushed down in the silt to the depth of 26 feet, and a temperature of 77° recorded. Whilst no doubt the first readings were rather too high, as the heat caused by the tools could not have been all lost,* it is equally probable that convection currents may have cooled the mud in the bore-hole. Taking, however, the temperature in the bore-hole between the depths in it of 100 and 670 feet (1166 and 1736 feet below surface), we have a difference of 11°, which gives a rate of increase of 52 feet per degree.

The strong ventilation, and the uncertainty about the length of exposure, are objections to the otherwise careful records of the Pendleton Colliery (No. 104), and the same doubts attach to the other deep works of this district. It is true that many of the observations are said to have been made in newly-opened ground, but we do not know whether this means an hour, a day, or a week, and as in all newly-opened mines the ventilation must be well kept up for the sake of the workmen, the rate of cooling must be rapid.

The Wakefield pit (No. 120) had been newly opened. Ventilation was stopped as much as possible at the stations, and the temperature of the air was very near the normal; the hole in the coal was 6 feet deep, and distant 600 yards from the shaft. In the Barnsley pit (No. 119) the station was too near the shaft, and the air was much below the normal.

The observations, both in the Radstock and Kingswood Collieries (Nos. 163 and 155), were made in holes only 2 feet deep, and the thermometers allowed to remain respectively two, five, and seven days—long enough for the rock to cool, as shown by the fact that after another week the thermometer at Kingswood gave the same reading. The temperature of the air is not given.

I pass over the Upper Duffryn, Tredegar, Cwyn Neol, and other

* The boring was only stopped about twenty minutes before the experiments were made.

collieries, in consequence of the deflections of the isothermal lines by irregularities of the surface.

The particulars of the Norley Colliery, Wigan, (No. 111) are insufficient, though the result there obtained seems to confirm the observations at the Rosebridge Colliery.

The observations in the Mons Collieries (Nos. 215, 216) were made at a distance from the shaft, and in dry galleries without ventilation.

At Liège (No. 141) the unusual precaution was taken of making the holes 5 mètres deep, while the temperature of the air in the galleries was only 1° lower than that of the coal.

I have not taken any of the earlier observations, owing to the uncertainty attaching to the instruments, and, after eliminating others for the reasons named, we are reduced to the following small number of available observations:—

Original number in Table I.	Place.	Depth of pit.	Temperature of rock or coal.	Rate of increase for 1° Fahr.
		feet.		feet.
150	Boldon, Newcastle	1514	79°	47
136	South Hetton, Durham—the last 670 feet.....	1736	77	52
126	Rosebridge, Wigan.....	2445	94	53
217	North Seaton.....	620	61	45
120	Wakefield.....	1455	78	50
141	Liège, Belgium	1656	87	46
216	Mons, Cuesmes Colliery.....	1680	82	54
215	„ La Louvière Colliery	1456	81	49
		Mean rate of increase....		49·5

This seems a very small selection to make, but I believe that the disturbing causes I have mentioned operate so constantly, that an element of uncertainty is introduced at the very opening of a pit, and that, without further precautions than hitherto taken, the observations are not reliable for the purpose of those exact values necessary to determine the true thermometric gradient.

Instead of making the observations in a newly-opened and well-ventilated part, with fresh faces of the coal emitting pent-up gases, it might be desirable to try an isolated abandoned unventilated gallery, and there place thermometers in the coal and the rocks above and below the coal. The holes should be 5 or 6 or more feet deep, and the gallery then closed at distances of 200 to 300 feet by a series of diaphragms to stop ventilation and convection currents, and the place not opened for several months, by which time the end station would

possibly have acquired the temperature of the circumjacent body of strata; or the place might be bricked up.*

It is only in a few instances that temperature observations have been made in carboniferous strata in other than coal-pits; in these others there is no indication of the excessive range which the observations in shafts and galleries have shown. The following table gives the results obtained in bore-holes sunk in search of coal. At Creuzot there is a thick capping of Triassic strata, and the coal strata dip rapidly.

Original number.	Place.	Depth.	Rate of increase for 1° F.
		feet.	feet.
123	Blythswood	347	52
124	South Balgray	525	42
68	Creuzot, Torcy Colliery	1817	57
69	„ Mouillonge Colliery.....	2677	52
		Mean	50·8

If we were to unite these two series of observations, we should get for the Coal-measures a rate of increase of temperature as nearly as possible of 50 feet per degree Fahr., although there seems to be local variations dependent upon structure, the percolation of water, and other causes.

II. Mines other than Coal. (*Table III, p.* 96.)

The causes affecting the thermal conditions of Metalliferous Mines are very different to those which obtain in Coal Mines. In them we have to deal almost exclusively with crystalline and schistose rocks. Not only does the conductivity of such rocks differ materially from that of the Coal-measures, but the disturbing causes common to the two have in general very different values.

Ventilation is a cause affecting both, but it affects them unequally; and while in Coal Mines the influence of water is generally very small, it plays an important part in Metallic Mines. On the other hand, chemical decomposition and hot springs, which are common disturbing causes in the latter, are of rare occurrence in the former.

Ventilation.—I have so fully described the effects of ventilation in the section on Coal Mines, that I only need mention here in what respects its effects differ in other mines. Besides, we are not in possession, in respect to metallic mines, of such detailed particulars as those which the Report of the Coal Commission gives of the Coal

* Or, deeper holes and well distant from the shaft might answer.

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Mines. The effects are modified in the two cases by differences in the structure of the shafts and galleries, and while in the one the presence of the coal-gases necessitates excessive ventilation, in the other, hot springs and chemical decomposition, though they raise the temperature, do not render so active a ventilation imperative.

In some of the Cornish mines the current of air is hardly felt, and it is stated generally by Mr. Robert Were Fox,* that in deep mines the temperature of the rock and that of the air do not materially differ, except when the currents are strong. At the same time it is evident that strong currents of air do sometimes prevail. An instance is mentioned by Mr. Robert Hunt,† where at the bottom of a mine 1950 feet deep, the current of air was so strong that it was difficult to keep a candle alight. In another, in all the levels (which were of great extent) of one hot lode at the depth of 1410 feet, the ventilation was so effective that the temperature of the air was never higher than 70° F.

These mines must suffer, as in coal mines, a great and permanent reduction of temperature in the proximity of the down shafts. This is especially noticeable in severe winters, and when the ventilation is active the effects of this will extend to a considerable distance from the shaft. Daubuisson mentions that during a severe winter, and with the outer air at a temperature of −15° R. (−2° F.), the shaft of the Beschertgluck mine, in the Freiberg district, was lined with ice to a depth of 80 toises (480 feet), and that the temperature of the air at the bottom of the shaft was ½° R. (33° F.). Dr. Clark ("Travels in Scandinavia,") says that in descending the great iron mines of Persberg, in Sweden, which are 450 feet deep, he found large masses of ice covering the sides of the walls, and that it had accumulated in large quantities in the lower chambers. But the formation of ice is not confined to these more rigorous winter climates, for Mr. Moyle speaks of finding, on one occasion, ice in abundance in the Tin Croft Mine, Cornwall, at a depth of 318 feet below the surface, and says that the ladders became impassable, crevices were filled, and icicles formed all around! That this must produce a permanent effect is clear from the circumstance mentioned in connexion with coal mines, where the walls of deep mines near the shaft are so cooled in winter, that in summer the air circulating by them is of a higher temperature than the rock. On the other hand, the temperature of the air in a metallic mine is more apt than in a coal mine to become heated by the combustion of candles, the explosion of gunpowder, and the presence of the workmen.

Some of the observations of Mr. Fox show the effects of ventilation in the Cornish mines. Thus at Huel Damsel Mine (No. 21), the tem-

* Coal Commission Report, vol. ii, p. 211.

† *Ibid.*, vol. ii, p. 87.

perature of the air at different levels varied very little, and was higher in the third level than in one lower.

Depth.	Temp. of air.	Depth.	Temp. of air.
480 to 540 feet	69°	780 to 840 feet	73°
600 „ 780 „	70	840 „ 900 „	70

That the temperature of the air does influence that of the rocks is shown by another series of experiments made by him in holes in the rock in Dolcoath Mine with the following results:—

	Depths.	Temperature of rock.	Rate of increase per 1° F.
I.....	240 to 300 feet	58°	34 feet
II.....	540 „ 600 „	58	71 „
III.....	720 „ 780 „	63	57 „
IV.....	1140 „ 1200 „	64	83 „
V.....	1320 „ 1380 „	78	48 „

Mr. Fox does not give the temperature of the air, but states that the abnormally low temperatures of Stations II and IV arose from the passage of strong currents of air. The effect of these conditions, in estimating the rate of increase of temperature with depth, is clearly shown in the last column which I have added for this purpose.

But although there may be extreme cases, it is probable, as a general rule, that the ventilation does not produce the extent of difference between the temperature of the air and of the rock that it does in coal mines. Mr. Fox who, to avoid the effects of ventilation, always, if possible, made his observations near the ends of the levels, states that in those cases there is little difference between the temperature of the air and the rock. At the same time it is possible that, even then, the uniformity may be owing to the rock having permanently cooled down to the temperature of the air—though it may not be much. The following are cases in which the temperatures of both are given.

	Depth.	Temperature. Air.	Temperature. Rock.	Rate of increase per 1° F.
No. 71. Par Consols	1248 feet....	82°	84°	38 feet
No. 72. Botallock	1128 „	81	79	40 „
No. 77. Levant	1530 „	85	85	45 „
No. 79. Tresavean.....	2112 „	91·5	90·5	52 „

Here it will be observed that there is only a difference of 1° to 2° between the temperature of the air and the rock, and that the rate of increase with depth is, with one exception, much more uniform.

May not circumstances such as these account for the marked dis-

crepancies in the rate of temperature with depth observed in the Talargoch Mine (No. 158, 160); for although the temperature of the air is not there given, we know that it will vary with the distance from the shaft, of which particulars are given in the Brit. Assoc. Reports:—

Station.	Depth.	Distance from shaft.	Temp. of rock.	Rate of increase with depth.
III....	660 feet	120 yards S.	54° F.	132 feet
V....	555 „	170 „ S.E.	52·9	140 „
IV....	465 „	190 „ S.W.	53·4	116 „
I....	1041 „	190 „ N.E.	60·8	88 „
VI....	636 „	840 „ S.W.	58·8	64 „
VII....	660 „	1240 „ S.S.W.	62	51 „

The two partial exceptions (IV, V) are in both cases at stations of lesser depths, and, being still near the shaft, are possibly more exposed to the influence of the outer air.

The effects of ventilation are also shown in another way. Some of the early experiments of Dr. Forbes were made in the water in *sumps* and pools, which necessarily would be much exposed to the cooling effects of the circulating air. His tables, which give the results of observations in six mines (No. 18), show a rate of increase of temperature relatively less with increased depth. The observations are so much at variance with those subsequently made by Mr. Fox and Mr. Henwood, that I can only attribute the discrepancy to the cause I have here named, viz., a relatively greater cooling of the water, due to increased ventilation at increased depths. It is not necessary to give the whole series: the following instances bring out the fact:—

Depth.	Temperature of the water.	Rate of increase per 1° F.
500 to 550 feet	65°	35 feet*
900 „ 950 „	71	44 „
1350 „ 1400 „	79	47½ „

The early observations of Mr. Fox, which were made in the same way in the water in fifty-three mines, gave also a diminished rate of temperature with increase of depth:—

Mean depth.	Rate of increase per 1° F.
354 feet	1° in 35·4 feet
448 „	1 „ 43·8 „
648 „	1 „ 64·2 „

* The rapid rate constantly observed near the surface is probably in great part owing to chemical decomposition in the lodes which is most active near the surface, and also to a less active ventilation.

No further particulars and no greater depths were given, but Mr. Fox's later observations in rock give different results.

The Effects of the Percolation of Water.—But while the effects of ventilation are not so general and disturbing in Metallic Mines as in Coal Mines, the effects produced by the underground waters are of much greater importance. The alternation of impermeable with permeable strata, and the multiplicity of faults in the Coal-measures, so impede the descent of the surface-waters, that there are mines so dry as to necessitate the introduction of water to keep down the dust. The Metallic Mines being, on the contrary, commonly in crystalline, schistose, and slaty rocks, have more uniformity of structure; and, being also generally hard and compact, they are more or less impervious. When, however, they have been disturbed and fissured, they give freer passage to water; and when, further, they are traversed by veins and faults, these frequently serve as channels or conduits, more or less free, for the surface-waters, and considerable quantities of water pass through them. Consequently water is one of the great obstacles to deep mining with which the workmen have to contend. Water finds its way to all depths, and with more or less rapidity. Mr. Henwood states that in some mines a great increase follows soon on heavy autumnal rains, and that in others, months intervene before the effects are felt.

In districts formed of the usual alternations of sedimentary strata, it is estimated that on an average about one-third of the rainfall passes underground; while in Cornwall, where granites and slates exclusively prevail, Mr. Henwood estimated in his survey of the Gwennap district—which consists chiefly of slates—that about a fourth of the rainfall is absorbed, the mean annual rainfall there being 46 inches, or equal to 166,834 cubic feet per acre. The local percolation is, however, extremely variable, as in some mines the quantity pumped up does not exceed 5 gallons, while in others it amounts to 186 gallons per minute.

The same observer found that water passes more freely through slate than through granite, the quantity yielded by mines in slate being about four times as much as that in granite. In a period of five years (1833–7) the water pumped per minute from forty mines, amounted on a mean to the following proportions :—

Granite.				Slate.			
Maximum.		Minimum.		Maximum.		Minimum.	
30	....	16	....	122	....	63	cubic feet per minute.

Mr. Henwood's account of the quantity of water that passes under-

ground in the Gwennap district is of much interest. The mines in that district, over an area of 5500 acres, were combined for drainage purposes. A great adit carried away the water above the sea-level, while a deep-seated level collected the water at the depth of from 1100 to 1200 feet. At the time that Mr. Henwood wrote, the former was discharging 1475 cubic feet per minute, and from the latter 909 cubic feet were being pumped up, or together above 10 million gallons in the twenty-four hours. Taking the mean temperature of the surface at 50°, as the water issues at a temperature of from 60° to 68° F., or at an average of more than 12° above the mean of the climate, it is easy to conceive how large must be the amount of heat which the waters abstract from the mines, and how considerable the cooling of the enclosing rocks which must result therefrom.

Another observer, writing a few years earlier, states that the discharge at the Huel Vor Mine from a depth of 950 feet, was 1,692,660 gallons every twenty-four hours; at Dolcoath Mine, from about 1400 feet, 535,173 gallons; and at Huel Abraham Mine it reached the large quantity of 2,098,320 gallons.*

Mr. Henwood remarks that "the largest streams of water flow through cross-veins; small ones through the lodes, whilst but little issues from the rocks whether granitic or slaty."

Where the water dribbles slowly through the rocks to great depths, it will no doubt acquire the normal temperature of the depth, but where it passes more rapidly through the veins and lodes, the temperature will depend upon the time occupied in transit and on the volume of water. If the flow is rapid, as it evidently is in some mines, the surface-waters may carry the influence of the above-ground temperature to considerable depths. If on the other hand, the vein is one in which the ore is subject to decomposition by the surface-waters, those waters will have their temperature more or less raised. A copious stream of warm water is considered among the Cornish miners a favourable indication of the proximity of a lode. Nevertheless, Mr. Henwood, who, as we may feel assured, fully understood all the contingent conditions, considered that by a careful selection of the underground springs and by taking them when freshly opened, they gave safer temperature results than did the undisturbed rock.

Hot Springs.—These are not uncommon in metallic mines. They are due to two causes. 1stly, to chemical decomposition; 2ndly, to water coming from greater depths.

The first of these causes is a very general one—especially in copper and iron mines, in which the lode consists of iron and copper pyrites. The surface-waters decompose these sulphides, converting them into sulphates, which by further changes that need not be here described, pass into the oxides and carbonates of these metals. That the action

* Dr. Forbes, "Trans. Roy. Geol. Soc. Cornwall," vol. ii, p. 167.

is general is shown by the circumstance that the upper part of all these lodes consists near the surface of a crust, several feet and sometimes several fathoms thick, composed of the oxidised products of copper and iron sulphides; this part of the vein is known in Cornwall under the distinctive term of *gossan*. In these cases, the water is commonly impregnated with some of the resulting soluble sulphates, and has its temperature raised by this decomposition.

Mr. R. Hunt mentions* two marked instances of the heating effects arising from this cause. In one case the temperature in the level of a copper mine stood at 100° F., but on the removal of a very large deposit of the copper pyrites "the level became cold enough to make the agent wish for a great-coat." The exact difference is not given. In another case, a large deposit of iron pyrites was opened at about half a mile distant from a hot lode, 1530 feet deep, in the Clifford Amalgamated Mine, and the mere fact of opening the mine there and removing the iron pyrites, considerably reduced the temperature in the mine. When it was closed the temperature rose to its former height. Springs of various degrees of heat (one was as high as 124°) are often met with.

The miners of Cornwall have long held that the lodes containing tin are, at equal depths, colder than those in which copper ores occur; a fact which is no doubt due to the facility with which the cupreous pyrites decomposes.

There must also be cases in which water from greater depths is brought up along lines of fissure (lodes and cross-veins); for as these are prolonged downwards, they may traverse at greater depths strata, veins, or faults, charged with water, which, when thus tapped, will outflow at any levels lower than the height at which the water stands in the supplying source.

It sometimes also happens that at the same depths, but in distant parts of the same level, the water is of different temperatures. In one instance there were springs at 102°, 110°, and 124°, and in another case (Wheal Wreath), the temperature of a small stream at the east end of a lode (tin) was 71°·5, while a spring at the west end of the lode had a temperature of 75° (both being at the depth of 1422 feet).

On the other hand, when the water is in considerable volume, and percolates rapidly, it must tend to have a lower temperature than the normal rock temperature, as in the instance where, in two adjacent mines, large streams both coming out of veins, had the same temperature of 67·5° at the respective depths of 588 and 722 feet. The observations of Henwood likewise tend to show that the range of temperature of water in the same level is subject to great variation.

In consequence of the uncertainty attaching on the one hand to

* Coal Commission Report, A 4—10.

observations taken in rock, and on the other to those in water, Cornish geologists have been divided in opinion as to the best plan to adopt. The two great authorities on the subject, Mr. R. Were Fox and Mr. W. J. Henwood, respectively gave preference,—the one to rock and the other to water (or rather springs),—and we may feel sure that the subject was well weighed and considered by both. Either system is in fact open to some objections, and neither can be employed without recourse to those safeguards and precautions, which no one knew better how to use than these two experienced observers.

Mr. Henwood gives as his reason for preferring springs,—that the rocks forming the sides of the shaft and levels must, to a certain extent, partake of the temperature of the air circulating through them, and that this air could not escape the influence of heat-producing causes at one time, and at another of the cooling effects of the intake-air. For the same reason he objected to the use for temperature purposes of the water standing in the levels, or *sumps*. He found it difficult to select stations at which the influence of those conditions would be always alike, and he was led to confine his observations as much as possible to the temperature of the streams of water immediately as they issue from fresh opened unbroken rock,—before they could be affected by the temperature of the levels,—as the places which would give the most correct temperature readings.

The following observations appear to me amongst the most reliable of those obtained in springs. The deepest seated springs are probably the most free from interference by the surface temperature and other disturbing causes.

No. in Table.	Mine.	Depth.	Temp.	Therm. gradient.	Spring in rock or lode.
		feet.	Fahr.	feet.	
52.	St. Ives Consols	810	71°	41	*Rock.*
49*f*.	Wheal Trenwith	660	66	44	*Lode.*
49*g*.	South Roskear	834	71	40	*Lode.*
49*a*.	East Wheal Crofty	810	70·7	39	*Rock.*
79.	Tresavean	2112	93·5	48·5	*Rock.*
27.	Dolcoath	1440	82	45	*Lode.*
49*b*.	Consolidated	1764	92·5	41·5	*Lode.*
49*c*.	United Mines	1080	74	45	*Lode.*
49*l*.	East Wheal Virgin	1722	94·5	39	*Rock.*
51.	Devonshire Wheal Friendship.	810	69·5	41·5	*Lode.*
			Mean..	42·4	

Instead of a few selected observations, Mr. Henwood took a larger number and a wider range, though to this there is the objection that he thereby includes a number of extreme and doubtful cases. Thus,

he took the large number of 415 observations made in ten Cornish mines, and on reducing them to given mean depths, obtained the following rate of increase of temperature with depth:—

	Average depth.		Mean temp. at depth.		Rate of increase per 1° Fahr.
i.	180 feet	....	54·8°	....	36 feet
ii.	432 „	....	60·8	....	40 „
iii.	762 „	....	67·4	....	43·5* „
iv.	1050 „	....	78·0	....	40 „

He elsewhere gives the mean temperature for other depths:—

Mean depth.		Mean temp.		Rate of increas
672 feet	....	66·9°	....	40 feet
1440 „	....	85·5	....	40·5 „

He noticed also that there was a difference between the temperature of the springs issuing from the granite and those from the slate rocks. The mean of 134 observations made to a depth of 1200 feet in Cornwall and Devon, gave the following thermometric gradients—

In granite	41·5 feet per 1° Fahr.
In slate	39·0 „

Further, taking separately the springs issuing from veins of different characters, he arrived at the following results:—

For cross-veins at a depth of	594 feet	....	40·0 feet	per 1°
For lodes generally	„ 660	„	41·6	„
For copper lodes	„ 840	„	38·0	„
For tin lodes	„ 552	„	51·5†	„

The result of these and various other calculations, is that for the springs issuing from the rock, the mean thermometric gradient was 40·1 feet, and from the lodes 40·3 feet per degree.

The early observations of Mr. Fox were made in the mine waters, but his later experiments were made in the rocks, and he expressed afterwards some doubts as to the value of his earlier experiments.‡ He considered that the rock observations were free from the direct influence which the descent of the surface-waters exercises on the lodes

* I am unable to explain this difference. The more rapid gradient in No. 1 is probably due to the heat resulting from the oxidation caused by the surface-waters

† There would appear to be some mistake in this figure, as in another page he states that the temperature in the tin and copper lodes conjointly at a mean depth of 444 feet is 61·4°, which gives a rate of increase of 39 feet per degree.

‡ Some were made in still water or *sumps*; others in the air of the levels.

and cross-veins—of which the effects may either be to raise the temperature of the underground springs when chemical decomposition is going on in the upper part of the lode, or to lower the temperature when the surface-waters are abundant and infiltrate rapidly.

The following observations made in the rock appear to be amongst the most reliable of the results obtained, the temperature having been taken in holes 2 to 3 feet deep, and the stations having been selected at places where no working had recently been going on, and as distant as possible from the shaft:—

No. in Table I.	Name of mine.	Rock.	Depth.	Temp. of rock.	Rate of increase for 1° F.	Observer.
			feet.		feet.	
19	Dolcoath	Granite	1350	78°	48	Fox.
76*a*—77	Levant	Granite and slate	1530 „	87 85	43·7	„
72	Botallock	Granite and greenstone	1128	79	40	„
80	Fowey Consolidated	Slate	1728	93	41	„
71	Par Consols...	„	1248	84	38	„
40	Consolidated..	Slate	1740	85·3	49	Henwood.
122*a*	Tresavean	Granite	2130	99	43·5	Hunt.
78	Tresavean	„	1572	82·5	48·5	Fox.
					Mean..44 feet	

This gives a mean thermometric gradient of just 44 feet per 1° F., or combining the results obtained by observations in springs with those in rocks, we get an average gradient of 43·2 feet per degree.

Mean of observations in springs	42·4 feet
„ „ rock	44·0 „
Mean..	43·2 „

Foreign Mines.—We know too little of all the conditions which obtain in foreign mines to draw any definite conclusion as to the rate of increase of temperature. Possibly the observations may prove available when we have more certain information respecting the mean annual temperature at each place, the height of the mine above the sea, and the position, especially in mountainous districts, of each station with reference to its depth beneath the surface.

This latter element is one, which the coal-mines observations previously given, clearly show should be taken into account. In Cornwall it is not of much importance, as the elevations are small, and the

mines are rarely more than 100 to 300 or 400 feet above the sea level; but the mines of Freiberg are on higher ground, whilst those of Prizbam and Chemnitz are situated amongst high hills, and the temperature at the end of a long gallery may be, in relation to depth beneath the surface, very different to that given by the depth of the shaft.

The mines of Freiberg are those in which the greatest number of observations have been made, though none of them are of very recent date. The deepest mine, and one in which the observations were made in holes in the rock (gneiss), gives, if we are right in our estimate of the surface temperature,* a rate of increase with depth of 54 feet per degree. But in this instance,—as in the case of Mr. Fox's observation (No. 73) in Dolcoath Mine, where the thermometer was left in position for one and a half year,—it was here left two years, and there must have been as before described, a cooling of the rock, such as would reduce the temperature so far below the normal, as to place it in equilibrium with the temperature of the cooler air in the galleries. The temperature of 66° at Freiberg, and of 76° at Dolcoath, with their thermometric gradients of 54 and 53 feet, I take to represent the normal temperature of the rock, minus the loss of heat due to ventilation; and if that can be estimated at anything like the loss shown to have taken place in a level in the Wheal Vor Mine, which, after it had been opened some time and the mine had been deepened, amounted to about 6°, or in a similar instance mentioned by Mr. Henwood, where the difference amounted to 7°, it would indicate a normal temperature of these mines more in accordance with the gradient which we have adopted on other grounds.

For the same reason the numerous observations made by Mr. Henwood in the mines of Brazil and Chili, are, for the present, not available, though it is possible that they may be rendered so at a future period, should the other factors necessary for determining the difference between the surface and the underground temperature be ascertained.

III. Artesian Wells and Borings. (*Table IV, p.* 106.)

This class of observations presents results much more uniform than those taken either in Coal or Mineral Mines, and whereas the observations in Mines were in Palæozoic or crystalline rocks, those in Wells are, with few exceptions, either in Cretaceous, Jurassic, or Triassic

* This is based on the temperature of Dresden, the nearest place where we have recorded observations. The mean annual temperature of Dresden is 47°, and as the mine is situated at the height of about 1300 feet, or about 900 feet above Dresden, if we allow 1° for every 300 feet of elevation, we shall have about 44° for the surface temperature at the mine.

strata, where the rocks are, as a rule, softer, more permeable to water, and less disturbed.

Many of the interfering causes difficult to eliminate in the case of the Mines, do not exist with the Artesian Wells. The causes of interference in the latter are reduced mainly to two, namely, pressure on the instruments and convection currents. The early experiments, where no precautions were taken against pressure, are consequently unreliable. Walferdin introduced improved and protected instruments, although in some previous cases, as in Marcet and De la Rive's observations, protected thermometers had been used. The need of protection against convection currents had also not escaped attention, but it was not until the later observations, instituted by the Committee of the British Association, were made, that more efficient safeguards were introduced to protect more completely against the subtle influence of these currents.

It is clear that we must reject all the early experiments made with unprotected thermometers; and it is not certain whether also a large number of those made with protected thermometers, but without protection, and sufficient protection, against convection currents, should not also be rejected.

In large bore-holes the disturbance from this cause is so great that the consequences are at once sufficiently apparent to oblige the rejection of the observations. The ordinary deep artesian wells of Paris, such as Grenelle and others, all agree in showing a rate of increase of 55 to 58 feet per degree F.; but the great bore-hole ($4\frac{1}{4}$ feet in diameter) through the same strata in another part of Paris (La Chapelle St. Denis), where the water does not overflow, gave a rate of increase of $39\frac{1}{2}$ feet per degree for the first 100 mètres (328 feet), which is too rapid, whilst at the depth of 660 mètres (2165 feet) the rate of increase was only 1° in 84 feet. This is clearly due, as stated by Professor Everett, to the overheating of the upper part of the column of water and to the cooling of the lower part by the action of convection currents. In the report of a deep boring at Moscow it is stated, but without explanation, that from 350 feet to the bottom at 994 feet the temperature was nearly constant at 10·1° C. (50·2° F.). The diameter of the bore-hole is not given, but I judge from other circumstances that it is not less than 2 feet, and can only account for the uniformity of temperature by the action of convection currents. The mean annual temperature of Moscow is 39·4°.

Professor Everett* also directs attention to the manifest action of convection currents in a shaft at Allendale, about 350 feet deep, with nearly 150 feet of water, in which the temperature was practically the same at all depths.

* Brit. Assoc. Reports for 1871 and 1869.

Depth.		Temp.
160	feet.	47·5°
200	,, ...	47
250	,, ...	47·7
300	,, ...	47·7

One of the pits at Allenheads (No. 130) which was nearly full of water gave similar results.

Depth.		Temp.
50	feet.	47·2°
100	,, ...	46·8
200	,, ...	46·6
350	,, ...	46·9

While in another shaft at Ashburton 620 feet deep, and with water standing to within 50 feet from the surface, the temperature at all depths was 53°, except at one point, where it rose to 53·4° and 53·2°.

Even in the deep and narrow bore-hole at Sperenberg, it was shown that the first experiments with thermometers protected against pressure, but not against convection currents, gave wrong results, for in the first instance at a depth of 100 feet, the temperature was found to be 11° R., and at 3390 feet, 34·1° R. Afterwards, when plugs were inserted to stop the currents, the temperatures at the same respective depths had altered to 9° R. and 36·16° R., showing therefore that the first readings were too high by 2° Reaumur near the top of the bore, and too low by 2·05° Reaumur at the bottom. Another experiment in the same well later showed that the difference at the bottom, between plugging and no plugging, was as much as $6\frac{3}{4}$° Fahr.*

Another narrow bore-hole, showing clearly the action of convection currents, was that sunk to a depth of 2000 feet, at Swinderby, Lincoln (No. 143).† The hole had remained undisturbed for nearly three weeks, and the water stood within a few feet of the top.

Depth.		Temp.
100	feet. . ..	68° F.
300	,, ...	68·75
500	,, ...	68·75
600	,, ...	69
900	,, ...	69
1200	,, ...	69·5
2000	,, ...	79

Taking the mean annual temperature of Swinderby at 48°, this

* Brit. Assoc. Report for 1876, p. 205, and 1882, p. 3. The differences were even considered to be under-estimated.

† Brit. Assoc. Reports for 1875 and 1876. Part only of these are in Table I.

gives a difference of 31° for the whole depth, or 64½ feet increase for each degree F. If it stood by itself this might appear nothing remarkable, yet it is evident, from the series of observations that were taken in this bore-hole, that the temperature or rate of increase at the depth of 100 feet, which was equal to 1° in 10 feet, and at 300 feet to 1° in 28 feet, is excessive, and that this excess can only have been acquired through loss of heat by convection currents at the bottom and a corresponding gain at the top, and thus making the readings too high at top and too low at bottom. If, however, we take an intermediate station at a depth of 1000 feet, where the temperature averages 69·2°, we get a mean, and more probably truer, rate of increase with depth of 47 feet per degree.

Owing to the large diameter of the bore, to the many water-levels in the rocks, and to the circumstance that when the observations were made there was no sufficiently free outflow of water on the surface, such as would check the convection currents, there can, I think, be no doubt that the temperature of the well at Bootle (No. 153), sunk in the New Red Sandstone at Liverpool, is influenced both by convection currents and by the influx of waters at different levels.

It is clear, therefore, that great uncertainty attaches to all observations made in bore-holes with standing water, the error being in proportion to the diameter of the bore-hole; and that where experiments have been made without plugging, all the deep temperature readings will be too low. Even with this precaution, it may be a question whether the bottom water and that of the adjacent rock may not have had their temperature permanently lowered before trial. This might be remedied to some extent by the use of more than one plug, and the stoppage of the circulation throughout the whole depth of the bore-hole for a certain time.

There are, however, some artesian bore-holes, where the sources of error have been reduced to a minimum. Amongst those are—

1. *Kentish Town* (No. 129).—These careful experiments were carried on for some years by Mr. Symons. From the circumstance that the mud at the bottom of the bore-hole into which the thermometer was sunk, was free from convection currents, the results obtained show, in all probability, a near approximation to the normal temperature. At the same time the long period that the well had stood neglected allowed the play of those currents in the water standing in the tube above the mud, and this may possibly have effected a slight reduction of temperature, but it cannot be very material.

2. *Richmond.*—The observations here were made by Professor Judd with standard instruments. The overflow was too small to give the correct temperature, though enough to check convection currents, and consequently the temperature was ascertained by letting down a thermometer to the bottom of the well.

3. The first experiments at *Grenelle* were made when the bore-hole had reached the depth of 400 mètres (1312 feet), and when no work had been going on for some weeks. The thermometers, of which three sets were used, were left down thirty-six hours. They were protected against pressure, and Arago remarks that the chalk through which they were boring made so thick a paste filling the bore-hole, that convection currents were hardly possible. All three sets of instruments gave results within a fraction of a degree to one another.*

4. In the bore-hole of the well at the *École Militaire* the experiments were made by Walferdin under very similar conditions.

5. *Pregny near Geneva.*—The thermometer was here protected against pressure, and to a certain extent against convection currents. The bore-hole was small, and the water stood at a small depth below the surface.

6. The observations at the well at *Ostend*, with the water also slightly overflowing, were made by Professor G. Dewalque, of Liège, with protected thermometers at the bottom of the bore-hole.

7. At *Sperenberg*, especial care was taken against convection currents, though how far these currents may have operated in reducing generally the temperature in the lower part of the bore-hole, before the experiments commenced, may be a little uncertain.

These seven wells give a mean rate of increase of rather more than 51 feet for each degree F. in depth (see p. 40). The reason why I have not included a larger number, is because in these cases I believe in the existence of some undetected errors, such, for example, as, amongst many others, those mentioned in the following instances.

Swinderby.—I have already explained my objections in this case. Of the first series of observations down to a depth of 1500 feet, Professor Everett remarks "it is obvious that nearly all the temperatures are largely affected by convection," he considered the bottom temperature at 2000 feet as less likely to be vitiated by convection in consequence of the small diameter of the bore-hole.

[But if we take, as I have suggested, a mean depth and a mean temperature, I believe we should have in the thermometric gradient of 47 feet per degree, a nearer approach to the true normal at Swinderby. Even in cases where the temperature is uniform from top to bottom, and there appears to be no clue to follow, as in the instance with the Moscow Well (No. 135), where Professor Lubinoff records a temperature of 10·1° C. (50·2° F.), if not for the whole depth of 994 feet, at least from 350 to 994 feet, it seems possible to obtain an approximate gradient. For the mean annual surface temperature being 39·5° F., and the half depth 494 feet, if we divide this by 10·7° (the difference

* I have adopted this observation for Grenelle, in preference to that on the overflow water, for the reasons given further on.

between the surface and the well temperature), we get a quotient of 46·5, which agrees nearly with the thermometric gradient of the well at St. Petersburg (No. 44). March, 1886.]

Southampton.—The well was too long disused; during that time convection currents operated; apparently no protection against convection currents during the experiment.

Bootle.—The influx of water at different levels, and convection currents, render the results here valueless.

Rouen, St. Sever.—The thermometer was not protected against pressure, but the effect of the pressure was estimated and allowed for.

Troyes.—M. Walferdin thought it probable that the observations were affected by the heat caused by the boring tools, sufficient time not having been allowed to elapse after working before the thermometer was sent down.

Artesian Wells and Bore-holes in which the Water stood below the Surface, or rose very slowly so as to render it necessary to take the temperature at the bottom of the bore-hole.

Original number.	Place.	Strata.	Depth.	Temperature.		Thermometric gradient.
				At depth.	Mean surface.	
			feet.	Fahr.	Fahr.	feet.
129	Kentish Town	Tertiary, Chalk, and Old Red Sandstone	1100	69·9°	*49°*	52·3
231	Richmond....	Tertiary, Chalk, Jurassic, &c.....	1337	75·5	*49·6*	51·5
37	Grenelle, Paris	Tertiary, Chalk, and Greensand.....	1312	74·7	*51*	55
36	École Militaire, Paris	Tertiary and Chalk..	568	61·5	*51*	54
29	Pregny	Tertiary (Molasse)..	713	62·7	*47*	48·5
207	Ostend	Tertiary and Chalk..	981	71·6	*50*	45
144	Sperenberg...	Triassic rock salt and Gypsum	E. ft. 3490	115·5	*48·3*	52
					Mean	51·2

Overflowing Artesian Wells.—We now come to surer ground. Under certain conditions, these wells must give not only the nearest, but a very near, measure of the underground temperature, at the level from which the water rises. These conditions are—

1st. A sufficient depth and a sufficient distance of the out-crop of the water-bearing strata from the point of overflow. In a case like the Grenelle well, which is nearly 2000 feet deep, and where the stratum which serves as a channel for the water does not come to the

surface for a distance of above 100 miles, these conditions are most favourable. It is very interesting to find, from information which M. Daubrée has just (Dec., 1884) obligingly obtained for me, that the temperature of the water at the Grenelle well is now precisely the same (27·8° C.), if not 0·1° C. higher, than it was at the commencement of the overflow forty-two years ago. In London, the distance of 10 miles at which the Lower Tertiaries and the Chalk out-crop is also no doubt sufficient, but it is a question whether the excessive pumping and constant passage of water may not have cooled the channels.

2nd. A sufficient volume and consequently rapid upward flow of the water in proportion to the depth, otherwise the water will part with some of its heat, as it rises through the tube. This may possibly be in some small degree the case at Grenelle, for in the carefully conducted observations of Arago and Walferdin, when the well had reached a depth of 400 mètres, the temperature (23·75° C.) gave a rate of increase of 55 feet per degree, instead of that of 58 feet when it overflowed. At the École Militaire, the rate was, as ascertained by Walferdin, 54 feet. It is possible, therefore, that the slower rate of increase at Grenelle indicated by the overflow water at a greater depth (548 mètres) may arise from a reduction of temperature due in part from the water overflowing with a velocity by no means great, but mainly from the diameter of the tube at the top being considerably greater than at the bottom.

The conditions of depth and volume are generally satisfactory in the case of the great saline wells in Germany, especially in the instance of the well at Neu Salzwerk. Humboldt states the supply of water was very abundant, the outflow being then at the rate of 422,000 gallons daily. Its ascensional force was remarkable, as was also the enormous discharge of carbonic acid. The deep artesian wells of Minden, which derive their water from the same source, give very similar results.

The discharge of water at the thermal springs of Mondorff in Luxembourg is very much less than at those of Neu Salzwerk. It is situated further south, and the height of the ground is considerably greater, being 673 feet above the sea-level; the mean annual surface temperature adopted by Walferdin was 48·4° F., but this was only an estimate. The mean temperature of Luxembourg, according to Arago, is only 8° C. (46·5° F.).

The depth of the artesian well at Tours is much less than that of the above, but the ascent of water was rapid and the discharge large. The observations there were made by Walferdin, as were also those at Rochefort.

The following list is confined to those wells where the overflow is abundant, and where the observations have been made by competent observers.

Artesian Wells in which there is an abundant overflow of Water at the Surface.

Original number.	Place.	Strata.	Depth.	Temperature.		Rate of increase per 1° Fahr.
				At depth.	Mean surface.	
			feet.	Fahr.	Fahr.	feet.
48*a*	Paris, St. Ouen	Tertiary.....	216	55·3°	*51°*	50
32	Lille.....	Cretaceous; Carboniferous limestone	329	57·2	*50·5*	49
31	Tours.....	Cretaceous	460	63·5	*53*	44
101	Rochefort.....	Trias	2812	111	*54·5*	50
65	Mondorff.....	Lias and Trias ...	1647	78·3	*47·3*	53
182	Minden	Triassic (?)	2230	90·9	*48*	52
34	Neu Salzwerk.	Liassic and Triassic	2038	88·3	*48*	50·5
144*a*	St. Petersburg.	Silurian.....	656	54	*39·17*	44
					Mean..	49·1

We thus have in these wells an average rate of increase of 49 feet for each degree F., or if we take the mean of the two lists, of almost exactly 50 feet per degree. Before, however, accepting this conclusion, two points should be reserved for correction hereafter, namely: 1st, the determination of the precise temperature at a depth where the annual surface changes cease to be felt. 2nd, whether there is any loss of heat, and what the amount, by the water in ascending through the tubes. This latter is, I believe, not an unimportant consideration.

The experiments at Grenelle indicate the probability of some such loss, and the form of construction of the tubes gives strength to the supposition. If the tubes of an artesian well were of the same diameter throughout, the passage of any given portion of the water from the depth to the surface would be direct, and the velocity the same throughout; but the tubes in almost all these wells decrease in diameter from the top to the bottom, consequently, instead of the whole body of water being in continuous and uniform motion throughout the length of the tube, the velocity of the water gradually decreases from the bottom to the top, currents are established at the points where the tubes enlarge, and a certain time beyond that required for its direct trajectory must elapse before the whole of the water issues at the surface.

For example, at Grenelle, where the difference between the size of the tubes at the top and bottom of the bore-hole is less than in some wells, there are four tubes of the respective diameters of 0·25 m., 0·22 m., 0·18 m., and 0·17 m. As the lesser the depth of the well the

fewer the changes of tubes, the differences in this respect, together with the differences in length of the tube, may possibly help to explain the reason why the wells of 300 to 600 feet deep gave generally a more rapid rate of increase than the wells of 2000 feet, and may also account for the different rates of increase at the several wells, for which otherwise there is no apparent cause.

I have excluded a large number of overflowing wells because of the uncertainty which attaches to the instruments used, or to some essential point of which we are in ignorance, such, for example, as the influx of other springs, the precise depth, the size of the tubes, &c. These reasons apply to such wells as those of Newport (No. 210), Falkirk and Midlothian (45), Dunkirk and Bourbourg (209, 208), Alfort (49), Meaux (62), Arcachon (183), and others, where we do not know whether or not standard and protected instruments were used, or whether the experiments were in all such cases made by competent observers.

With respect to the extra-European observations, still greater uncertainty attaches from our ignorance of the general conditions, and especially of the exact mean annual temperature of the several places. At the same time there are some exceptions worthy of consideration. The experiments in the Sahara Desert (No. 88*a*) were made by an engineer of great experience in the construction of artesian wells, and accustomed to observations of this description, and are the mean of results obtained at a number of wells. The observations at Charleston and St. Louis appear reliable, only in these cases more particulars are desirable.

The African and Indian experiments seem to indicate a more rapid rate of increase of temperature with depth than occurs in Europe. Not so the American (U.S.) observations, which appear to indicate conditions very similar to those which obtain here. Not much weight can be attached to the solitary observation in South America. It requires confirmation.

IV. Tunnels.

The few observations of this class, limited as they are, show not only the modifications of the gradients caused by inequalities of surface, but bear also on some important geological questions connected with the structure of mountain chains and metamorphism. The first great tunnel was that of Mont Cenis, which is about 7 miles long, and passes under a ridge of the Alps rising 9532 feet above the sea level, and 5280 feet above the tunnel. After making a correction for the convexity of the surface, Professor Everett estimates the rate of increase of temperature with depth to be 1° F. in 79 feet. But the observations there were commenced late, and were not very complete.

In the St. Gothard tunnel, where very full and complete observations were carried out by Dr. Stapff, the results are of much interest. The tunnel is about 9 miles long; the summit level of the ridge above the tunnel is 10,040 feet above the sea level, and 5578 feet above the tunnel. This, after allowing for the convexity of the surface, gave a rate of increase of 1° F. in 82 feet. But Dr. Stapff* has since pointed out that in one part of the tunnel the rate is considerably more rapid. He found that the relative temperature of the ground above the northern end of the tunnel was much higher than in other parts—that in the plain of Andermatt the mean rock temperature was several degrees above the normal, while at the south end of the tunnel it was some degrees below it. The latter circumstance was easily explained by the presence of cold springs; and some higher temperatures in other parts of the tunnel were attributable to the decomposition of the rock; but there were no apparent reasons for this excess of temperature in the northern end of the tunnel, where it passes through gneiss and granite. The difference was such that instead of a rate of increase of 1° in 85 feet in the centre of the tunnel, or of an average rate of increase for the whole tunnel estimated by him at 57·8 feet, the rate was here 1° F. in 38 feet. Dr. Stapff says that there is no obvious explanation of the rapid increase in the granite rocks at this end of the tunnel, and that it is probably to be attributed to the influence of different thermal qualities of the rock. He mentions, further, that this granite belongs to the *massif* of the Finsteraarhorn, which is of a different (newer) geological age to that of the central axis of St. Gothard, and that it is therefore not to be wondered at "if one of them be cooler than the other." He elsewhere remarks that there is also a well-known local focus of heat (decomposition of rock) below the valley of Andermatt, which may exercise a sufficient influence.

I myself am disposed to attribute the greater heat of these rocks to mechanical action rather than to the later protrusion of the Plutonic rocks, or to any subsequent decomposition of these rocks. If the pressure, force, and friction accompanying elevation of mountain chains be attended by the development of great heat—a heat sufficient to produce great chemical changes even in the Tertiary strata—then it may be possible for some of the newer mountain chains still to retain a portion of that heat. The facts brought forward by Dr. Stapff in the St. Gothard tunnel give material support to this view.

Although Mallet failed to show that the heat produced in the crushing of rocks by the lateral pressure, arising from the contraction of the crust of the earth as a consequence of its slow secular refrigeration,

* "Trans. North of England Inst. Min. and Mechan. Engineers," vol. xxxiii (1883), p. 19.

was sufficient to fuse the rocks and account for volcanic phenomena, he nevertheless brought prominently forward the enormous heat-producing power of the disturbances caused by this contraction. He made a series of elaborate experiments to ascertain the force required to crush blocks of a given size (3 or $3\frac{1}{2}$ cubic metres), and measured the work done by the *estimated* heat evolved by the crushing of 1 cubic foot of several classes of rock by the number of cubic feet of water at 32° F. converted into steam of one atmosphere, or 212° F. This method, although not perfectly satisfactory, is sufficient to prove the essential fact that a mechanical disturbance of the rocks may develop a large amount of heat. I must refer to his valuable paper* for full details of his results. The following is an abstract from his large table of experiments.

Class of rock.	Specific gravity. Water =1000.	Weight (pressure) per square inch at first yielding.	Mean pressure at which the cubes were completely crushed.	Temperature of 1 cubic foot of rock due to work of crushing.	Number of pounds of water at 32°, evaporated into steam at 212°.
		lbs.	lbs.	Fahr.†	lbs.
Caen Oolite	2·337	1,620	4,966	8°	0·288
Magnesian Limestone ..	2·571	3,699	16,333	26	0·9
Coal-measure Sandstone.	2·478	10,970	29,783	86	2·5
Devonshire Marble	2·717	11,708	34,938	114	3·44
Bangor Slate	2·859	15,510	41,590	144	4·51
Rowley Ragstone.....	2·827	24,039	63,737	213	6·86
Aberdeen grey Granite..	2·678	16,868	51,123	155	4·44
Inverary Porphyry	2·594	26,149	69,786	198	5·22

Thus with the ordinary sedimentary rocks the crushing weight (or that at which the blocks yield to pressure) is from $2\frac{1}{4}$ to $15\frac{1}{2}$ tons per square inch of surface, while for the crystalline rocks it rises to 31 tons. The heat produced on the metal surroundings by the crushing was in most cases easily perceptible to the hand, and was so great in some of the granites and porphyries as to necessitate a delay for the apparatus to cool. Both Mallet and Rankin were of opinion that "in the crushing of a rigid material such as rock, almost the entire mechanical work (with the small residue of external work) reappears as heat." If, therefore, the disturbance affecting the massive strata of a great mountain range were sudden or of short duration, an intense degree of heat might be rapidly developed; but there is reason to suppose that such movements have been of extreme slowness during

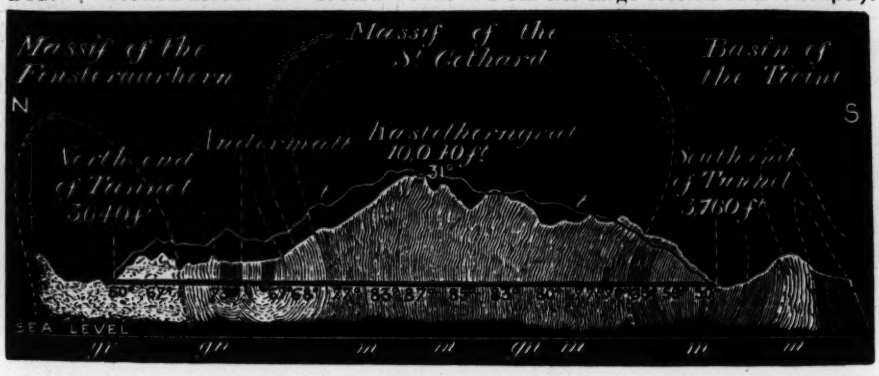
* "Phil. Trans.," vol. 163 (1873), p. 147.

† Omitting fractions of a degree.

long periods of time, and it was only when the tension had reached a certain point that fracture and disruption, accompanied by a more rapid motion of the parts, took place.

What the force of the pressure may have been in these cases is shown by the compression of the strata in the Alps, by the extraordinary folds and inversions of the rocks, and by the vertical cleavage (a resultant of pressure) which the whole mass of rocks has undergone. We may illustrate this point by the following generalised section across the central axis of the Alps along the line of tunnel.

FIG. 5.—Section across St. Gothard (reduced from the large section of Dr. Stapff).



gr. Granite. *gn.* Gneiss. *m.* Metamorphic schists. *t.* Temperature curves.

But although the compression may have been excessive, and the actual mechanical displacement great, the crushing was not so complete nor so sudden as to produce the extreme effects indicated by the experiments. Complete crushing is not, however, necessary for our object, since the experiments show that on the first yielding of the rocks, which takes place when the weight is rather more than one-third of the crushing weight, a large portion of heat is given off. Consequently not only would the heat be developed very gradually (and much of it might be dissipated during the long periods that the disturbances lasted), but also the major effects obtained artificially would never be realised in nature. Nevertheless, although fusion may not have taken place, there were molecular and chemical changes produced in the rocks which indicate the existence of very considerable heat; and there is reason to believe that this heat was often due to mechanical causes, and not to the protrusion of the molten granitic centres. In fact M. A. Favre and other Swiss geologists now consider the granite in those ranges to have been in existence in its present relative place when the elevation and crushing occurred.

Mallet further showed that the quantity of heat developed varied greatly in different rocks, and that, although compressed by the same force before their elastic limits were passed, yet, when released, it would render a quartz rock nearly three times as hot as a slate rock.

Consequently granite and gneiss, with their large proportion of free quartz, would be more affected than the other rocks.

When, therefore, we consider at how late a geological period some of the great mountain chains have been uplifted, it is not impossible, looking at the magnitude of the *massifs*, that some residual portion of the heat produced by compression, faulting, and crushing, may still exist in such modern chains as the Alps and the Himalayas, or in Continental areas of recent elevation when that elevation has been accompanied by compression and faulting. This is a consideration which, although exceptional, should not be overlooked in the general question of underground temperatures, especially in mining districts, where we have to deal with disturbed areas, with their faults, dykes, and mineral veins; at the same time, there can be little doubt that the disturbances in most of these areas are of such high antiquity that there is in most instances small probability of the rock showing remaining traces of thermal effects due to these causes. Nevertheless, when the area has been affected by late disturbances, it is possible for the thermic normal to be influenced by such a cause independently of the action of any volcanic or igneous rocks.*

Conductivity of the Rocks. Effects of Saturation and Imbibition.

Although it is evidently possible to account for many of the apparent discrepancies in the thermometric gradients by the causes discussed in the foregoing pages, yet it is equally evident that there are irregularities—not only between the rocks in the three groups of observations, but also common to individual instances in each separate group—which these causes do not adequately explain. As the rocks in each group are of very different lithological characters, and as there are also occasional lesser differences of characters in the members of each separate group, the common disturbing cause may in some measure depend upon those differences of structure and composition which variably affect the conductivity of the rocks.

The researches which bear most directly on this inquiry are those of Professors Herschel and Lebour† in this country, and M. Jannettaz‡ in France, the former relating more especially to the differences dependent upon the lithological structure of the rocks, and the latter to those dependent on the component minerals. Tabulating the results of Professor Herschel and Lebour's experiments in accordance with their geological relations, as grouped in Tables II, III, and IV, we obtain the following mean conductivities for the several groups of rocks:—

* For a further discussion of this subject see a short paper by the author on "Regional Metamorphism," in "Proc. Roy. Soc.," vol. xl (1885), p. 425.

† The results of their investigation are recorded in the reports of the British Association for 1874–1882.

‡ "Bull. Soc. Géol. de France," 3rd Ser., vol. iii, *et seq.*

Table of Thermal Conductivities of Rocks, compiled from the Tables of Professors Herschel and Lebour.

Nature of rock.		Absolute thermal conductivity, k.	Absolute thermal resistance, r.	Average. k.	Average. r.
Crystalline and Volcanic rocks.	Granite (mean of five varieties).	0·00584	172	0·00562	178
	Porphyry (Germany) ..	0·00513	195		
	Porphyritic trachyte ..	0·00590	169		
	Basalt (Loch Katrine)..	0·00560	179	0·00475	221
	Trap (Calton Hill)	0·00352	284		
	Serpentine	0·00515	199		
Schistose rocks and Slates.	Gneiss (Germany)	0·00514	195	0·00531	190
	Mica schist (Scotland)	0·00520	192		
	Slates (three varieties)..	0·00561	184		
	„ (across cleavage).	0·00395	253		
	Clay-slate (two varieties)	0·00327	307		
	Quartzite	0·00954	105		
Sandstones.	Ganister sandstone	0·00630	159	0·00734	139
	Craigleith „	0·00947	105		
	Hard sandstones	0·00672	149		
	Micaceous flagstone (along cleavage).	0·00690	145		
	Micaceous flagstone (across cleavage).	0·00492	203	0·00323	252
	Soft red sandstone	0·00397	252		
	New red sandstone	0·00250	..		
	„ „ wet.	0·00600	166		
	Firestone (Upper Greensand).	0·00240	427	0·00172	689
	Quartzose sand.....	0·00105	952		
	„ „ wet	0·00820	122		
Limestones, Oolites, &c.	Statuary marble	0·00530	189	0·00561	180
	Devonian „	0·00645	157		
	Carboniferous limestone	0·00550	182		
	Magnesian „	0·00522	192		
	Oolite (Ancaster).....	0·00370	270		
	Lias (building stone) ..	0·00360	278		
	Chalk	0·00220	455		
Argillaceous strata.	Coal-measure shale	0·00235	425	0·00242	411
	Clay (sun-dried)	0·00250	398		
	„ (the same wet and soft).	0·00350	270		
Mineral masses.	White quartz	0·00957	104		
	Alabaster	0·00360	278		
	Rock salt	0·01280	78		
	Coal (Newcastle).....	0·00068	1470		
	Cannel coal	0·00120	787		

Or dividing this series into groups corresponding with the nature of the rocks in the several Tables of underground temperatures, the results are as under:—

	Mean conductivity. *k.*	Mean resistance. *r.*
1. Coal Mines (*carboniferous strata*). Sandstones..... Shales; Clays.....	0·00433	267
2. Mineral Mines (*metamorphic and crystalline rocks*). Crystalline rocks ..... Schistose rocks; Clay-slates...	0·00473	225
3. Artesian Wells (*mesozoic and tertiary strata*). Soft, and New Red, sandstones; Oolites..... Chalk; Greensands..... Clays; Marlstones (Lias)	0·00308	331

This shows a considerable difference between the conductivity of the metamorphic and palæozoic rocks of the Mineral and Coal Mines, and that of the newer strata in which the Artesian Wells are usually situated; but there are conditions, hydro-geological and structural, obtaining in the rock masses themselves, which introduce many modifications affecting the value of these differences.

For example, in each group there are subordinate beds, which may have more or less local influence. In the first there are seams of coal, of which the conductivity is extremely small, though these seams form but a fraction of the entire mass. Thus there are—

	No. of workable seams.	Total thickness of coal.	Total thickness of coal-measures.
In the coalfield of Newcastle	16	.... 46 ft.	.... 3,000 ft.
„ „ N. Staffordshire	30	.... 130 „	.... 5,000 „
„ „ South Wales ..	75	.... 126 „	....11,000 „

The coal therefore only enters as a very small fractional part into the constitution of the Coal-measures. Besides, the coal seams vary very much in thickness, and are of limited range, that is to say, they form thin plates never coextensive with the Coal-measures themselves; at the same time, when one thins out, it may be replaced by another on a different level.

In the crystalline and schistose rocks, veins and layers of quartz and of quartzite are of common occurrence, though very irregular in their mode of distribution. The quartz veins and seams are generally thin, but the quartzite often forms masses of large dimensions.

In the Artesian Wells gypsum and rock salt are of not unfrequent occurrence in the Triassic strata; in the remarkable instance of the

Sperenberg bore-hole, the latter forms a mass several thousand feet thick.*

The Influence of Water.—The above, however, are but local and minor conditions, subordinate to one of a much greater and wider influence. The conductivity experiments of Messrs. Herschel and Lebour were, with few exceptions, made with blocks of dried rock. In a few instances they repeated the experiment with *wet* blocks of the same material, and with a remarkable difference in the result. Thus—

	Conductivity.		
	Dry.		Wet.
New Red Sandstone	0·00250	.....	0·00600
Quartzose sand	0·00105	.....	0·00820
Clay	0·00250	.....	0·00350
Mean.....	0·00202	.....	0·00590

Here we have substances which when dry present great thermal resistance, becoming when wet amongst the best of the rock conductors of heat—equal, if not superior, to that of the crystalline and schistose rocks.

This condition becomes, in considering the question of conductivity in relation to underground temperatures, a matter of very great importance, for in nature dry rocks are the exception and wet rocks the rule. The level of permanent saturation of the strata is regulated by the sea level on the outside, and by the level of the river valleys and their tributaries inland. All the rocks below those levels are, as a rule, permanently saturated with water, while between the valleys the line of water level rises in proportion to the distance the water has to travel, and the friction offered by the rocks before it escapes as springs. In the chalk hills of Kent or Surrey, for example, which rise to the height of 500 to 600 feet, the water level stands 200 to 300 feet beneath the surface, while below that level, the rock, whatever its thickness, is in a state of saturation. In the valleys the chalk is saturated to the surface; and Artesian Wells are always in relatively low levels. It is the same with the sandstones of the Trias or of the Coal-measures, only that in the latter the presence of faults often cuts off the supply, and segments exist with but little water except that of imbibition. This water of imbibition, or quarry water, is present in rocks above the line of permanent saturation, it being a property that depends on the capillarity of the rock, which is very strong in chalk and oolite, while it is slight in quartzose grits and sands. There are therefore few rocks in which the influence of water is not felt.

* It may, however, be a question whether it is not intercalated with thin seams of gypsum.

The following is the proportion of water held in rocks by complete saturation :—*

	Complete saturation.
Granite (hornblendic)	0·06 in 100 parts.
„ fine grained	0·12 „
Basalt, Auvergne	0·33 „
Silurian slates, Angers.....	0·19 „
Devonian limestone	0·08 „
Coal shale	2·85 „
Coal-measures sandstone	14·30 „
New Red Sandstone.....	13·43 „
Inferior Oolite	23·98 „
Calcareous freestone, Paris ..	16·25 „
Chalk	24·10 „

In the hard granites, sandstones, and limestones, the water of imbibition differs but little in proportion from that of saturation. The difference is considerable in the softer rocks. The following are some of the few experiments that have been made on this point.

	Quarry water.
Gneiss, slightly decomposed	3·00
Plastic clay	19·56
Chalk	19·30

It is clear then that the conductivity of the underground rocks must, except in some very hilly districts, be taken solely as that of wet rocks. In the harder and more compact rocks there will be little difference, but in the softer and more porous rocks the difference arising from this cause must be very considerable.

The conductivity even of coal will be increased, although the quantity of water that coal imbibes is very small. But unwrought coal also contains a large proportion of gas—and gas in a state of extreme condensation, or possibly in a state near liquidity, and this also may have an effect upon its conductivity.

Foliation and Cleavage.—The other condition, to which we have already alluded, is that produced by foliation and cleavage, and by the angle at which the strata lie. Messrs. Herschel and Lebour showed, for example, that the conductivity of slates varies accordingly as it is taken across or along the planes of cleavage—that while the conductivity along the planes of cleavage is equal to that of the crystalline rocks, it is no greater than that of soft sandstones across

* These are on the authority of the late M. Delesse, "Bull. Soc. Géol. de France," 2nd Ser., vol. xix, p. 64.

those planes. The lamination in micaceous sandstones produces a similar result:—

	Conductivity.	Resistance.
Slates, along the planes of cleavage..	0·00561	184
„ across „ „ ..	0·00395	253
Micaceous flagstone, along the laminæ	0·00690	145
„ „ across „ ..	0·00492	203

M. Jannettaz* has extended the inquiry to a great number of other rocks, and he shows that the variation in conductivity in many rocks is largely dependent upon the presence of mica. He found that in a crystal of mica, heat was conducted about two and a-half times more rapidly along the planes of cleavage than perpendicular to it. In augite these axes of the thermic curve are in the proportion of about two to one.

Fig. 6.—Mica.

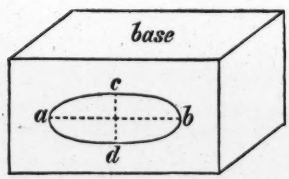
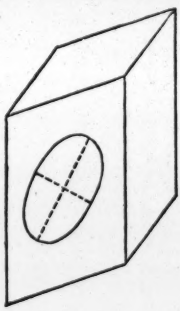


Fig. 7.—Augite (var. diopside).



a, *b*, *c*, *d*, the thermic curve; *a*, *b*, the major axis; *c*, *d*, the minor axis.

M. Jannettaz obtained results of a similar character, varying according to certain physical conditions, in a number of other minerals and in many rocks. The ratio of the minor to the major axis in the following rocks he found to be as follows:—

Gneiss of St. Gothard	1 : 1·50
„ from near Chamouni.....	1 : 1·23
„ passing into mica schist	1 : 1·63
Schists (triassic), St. Gervais.....	1 : 1·50
„ (carboniferous), Col Voza	1 : 1·80
Argillaceous schists	1 : 1·25
Cambrian Slate, Deville (Belgium)	1 : 1·86
Fissile micaceous limestone	1 : 1·31
Black and white limestone, Bonneville	1 : 1·06

* "Bull. Soc. Géol. de France," 3rd Ser., vol. ii, p. 265; vol. iii, p. 499, *et seq.*

The thermic curves attain their maximum variation in talcose and micaceous schists and in slates. The greatest inequality, 1 : 3, was shown by a specimen of a talcose rock, of sp. gr. 2·7. The variation exists in all rocks showing schistosity or lamination, but in ordinary stratified rocks the thermic curve remains that of the circle. It was found that the variation exists also in rock crystal, gypsum, felspar, &c. All the specimens experimented upon were dry.

It is evident then that in gneissic rocks and slates, the dip, cleavage, and foliation may have a very important effect on the conduction of heat; lamination has a similar but lesser effect in argillaceous shales; in ordinary sandstones and limestones no such effects are produced. Whilst these effects therefore may be very manifest in the rocks generally associated with Mineral Veins, they can only be small in Coal Mines, although they may be in some places increased by a larger proportion of mica in the sandstones and shales. There is also the further consideration with strata, such as those of the Coal-measures, that although there may be separately little difference in the thermic axes of the different rocks, the differences of conductivity in the various component strata may, as with foliation, allow of a variable transmission of heat along the planes of the inclined strata at their outcrop. But, even if that be the case, the effect would be merely local, possibly affecting the mass of inclined strata to a given depth, but in no ways affecting the special problem in relation to the general body of strata unaffected by those local conditions.

Conclusions.

The list of selected cases on which these conclusions are based may appear small, but I feel satisfied that the sources of error in experiments of underground temperatures are so many and so obscure, that without the fuller information which we have in these few instances, the larger number are not available for our purpose, though they all bear on the general question, and with the corrected data before named it may be possible to utilise some of them hereafter. We now require, however, for this object those observations only which give the nearest possible approximation to the true thermometric gradient, and for this it is necessary to reject all the doubtful and more uncertain cases. For these reasons I have confined myself, in the case of the Coal Mines, to the limited number of the eight instances given in the list at p. 24; and in the Mineral Mines to eighteen of the seemingly most reliable rock and spring observations of Fox and Henwood. The Artesian Wells give more uniform results,—results which, under certain conditions, should be perfectly true. I have, however, only been able to select fifteen wells, of which eight are overflowing wells, and seven not-overflowing.

Taking these three classes of observations we obtain the following values for their several gradients:—

	Thermometric gradient per 1° Fahr.
Coal mines.....	49·5 feet
Mines other than coal.....	43·2 „
Artesian wells	50·0 „

The mean of the three thus gives a general thermometric gradient of 47·5 feet per degree.

I do not, however, by any means consider this more than an approximation to the true normal gradient. In Coal Mines the effects of ventilation, and in other Mines the effects of chemical action and the circulation of water, have yet to be more accurately determined; while in the case of Artesian Wells, I believe the gradient of 49·5 feet may be too low in consequence of the unequal velocity of the water in deep overflowing wells, and of the uncertain measure of convection currents in those which do not overflow.

Admitting, however, these determinations to be approximately correct, they show that different geological areas have, in all probability, different gradients, and indicate possible inequalities in the underground isothermals, unless the altered conditions which come into play at greater depths tend to reduce and level them.

There is reason also to believe that the conductivity of the rocks at great depths may be affected by their hydrometric state and temperature. The descent of the surface water may ultimately be retarded or stayed by friction and heat. Faults, although they may stay its descent, leave untouched the water originally inclosed or imbibed. M. Delesse, who made some calculations on the probable depth to which water descends, concluded that water might circulate to the depth of about 8 miles before this limit was reached.

Further the experiments of Regnault determined the expansive force of the vapour of water up to a temperature of 239° C., the pressure then being equal to 27½ atmospheres. Beyond this, it has only been carried by empirical formulæ, but both experiment and calculation indicate that, with the increase of temperature, the increase of force is extremely rapid, and there is in all probability a point at which the vapour-tension of the heated water will equilibrate the hydrostatic pressure.

With respect to the possibility of change in the thermometric gradient at depths, it is known that the conductivity of wrought iron diminishes as the temperature increases, and at a rate agreeing very closely with the empirical law that the conducting power of iron for heat is inversely as the absolute temperature. What the variation in rocks may be has yet to be determined experi-

mentally;* we may presume it to exist, although it may differ materially in degree.

Therefore, taking into consideration the probable limitation of the percolation of water, and the possible diminution of conductivity with increase of depth, if there should be any alteration in the thermometric gradient, at great depths, it will be more likely to be in the direction influenced by these more or less certain factors, or in favour of a decreased conductivity and a more rapid thermometric gradient, rather than otherwise.

I have made a few attempts to ascertain, with the data in our possession, whether there exists any indication of such variation within the limits of the depths reached, by comparing the gradients of the upper with the lower portions of the mines, but without arriving at any satisfactory result.† It is true that in the Coal Mines, taking a depth below 1000 feet, the gradient, in all cases except two, shows, with increased depth, an increased rapidity, but it is a question whether this is not due to ventilation and convection currents causing too low a reading of the gradients in the upper part of the mines, and so throwing an apparent gain into the gradients in the deeper parts of the mines.

In the Mines other than Coal, some show at great depths a more rapid, and others a slower gradient, but it has to be observed that generally there is greater steadiness in the gradients at depths beneath 500 or 600 feet, than in those which are shallower.

In Artesian Wells and bore-holes, on the contrary, the gradient is often more rapid in the upper than in the lower section of the wells, but this is clearly due to the action of convection currents; while the decrease in the diameter of the bore-hole with the increase of depth, by unequally checking the flow of water, differently affects the temperature of the water in the tubes as successive depths are reached.

Looking, however, only at the more certain and determined causes which have interfered with the value of even the best observations, I believe that the effect of them has been to make the readings for the Artesian Wells and bore-holes especially, as well as the Mines, too low; and it may be a question whether a general average gradient of 45 feet per degree would not be nearer the true normal than the one of $47\frac{1}{2}$ feet obtained by the foregoing investigation.

* The large proportion of iron present in the deeper seated igneous rocks is an element to be considered.

† See the figures between the brackets and in italics in Tables II, III, and IV.

I. GENERAL TABLE OF

In this Table the observations are placed in order of date. The is generally due to the use of improved methods and instruments, or

All the observations are tabulated in the terms of the original papers, rected in accordance with later and better determinations. I am some of these corrected temperatures (marked *m*), and for others to "Notices Scientifiques" of Arago (marked *a*). In other cases they and additions have also been made for the surface heights. Where the place of observation is given, allowance being made for difference the other columns having reference to the same observations.

By an Artesian Well is meant a drill-hole carried down to a deep- face. By bore-hole is meant a boring of the like description, made

A few ordinary wells are introduced merely as guides to the mean

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
1. Giromagny, near Belfort..	*Copper and lead mine*..	*1535*	*47°?*
„ „ ..	„ „ ..	..	„
„ „ ..	„ „ ..	..	„
„ „ ..	„ „ ..	..	„
2. Bex, Switzerland.....	*Salt mine**	..	*45?*
3. Guanaxuato, Mexico.....	*Silver mine*.....	*6632*	*61?*
4. Cabrera „	„	*8510*	*60?*
5. Tehuilotepec „	„	*5776*	*63?*
6. Micuipampa, Peru.....	„	*11614*	*46?*
7. Freiberg, Saxony.....	*Beschertglück lead and silver mine*	*1378*	*43?*
„ „	„ „	„	„
„ „	„ „	„	„
8. „ „	*Himmelfahrt* „	*577*	*46?*
„ „	„ „	„	„
„ „	„ „	„	„
9. „ „	*Kuh-schacht* „	*656*	*45?*
10. „ „	*Junghöhe-Birke* „	*1050*	*44?*
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„

* The mine had been disused for three months.

UNDERGROUND TEMPERATURES.

discordance in those observations, which are repeated more than once,
to corrections of the mean temperatures of the place.
with the exception that the mean annual surface temperatures are cor-
indebted to Mr. R. H. Scott, F.R.S., of the Meteorological Office, for
the lists of the Scottish Meteorological Society (marked *s*), and to the
are those given by the original observers or by Dove. Corrections
temperature of the place has not been recorded, that of the nearest
in height, &c. (ante p. 7). (*a*) in Column VII refers to notes in

seated spring, which rises by that means *over* or *near* to the sur-
either in search of minerals or of water, and with or without water.
temperature at surface or at small depths.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
1	332	53·6°	Gensanne, 1740; Arago, "Notices Scientifiques," vol. iii, p. 317 (1856). Temp. of Mulhouse, 51°.
	675	55·4	
	1010	66·2	
	1420	72	
2	721	63·5	Saussure, 1796; Arago, *op. cit.*
3	1712	98†	
4	164	63	
5	358	75·5	Humboldt, quoted by Arago, *op. cit.*, p. 338.
6	1500 ?	67	
7	722	54·5	
	870	58	
	984	60	
8	328	50	Daubuisson. The original observations were published in the "Journal des Mines," vol. xiii, p. 113 (1803). The observations recorded here are given by him in his "Traité de Géognosie," 1819, p. 444, as the most reliable.
	590	54·5	
	870	58	
9	870	57	
10	656	57·2	Temp. of Dresden, 47° F.
	804	59	
	936	61	
	1082	62·5	

† Temperature of spring issuing from lode. The air of the working galleries was 92° Fahr.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
11. Brittany, Poullaouen	*Lead and silver mine*	*348*	*51°(a)*
„ „	„ „	„	„
„ „	„ „	„	„
12. „ Huelgoet	*Lead and silver mine* ..	*568*	*50*
„ „	„ „	„	„
„ „	„ „	„	„
13. Freiberg, Saxony	*Alte Hoffnung Gottes*..	*1300?*	*43?*
„ „	*Silver mine*	„	„
„ „	„ „	„	„
„ „	„ „	„	„
14. Whitehaven.....	*Coal pit*	*50—100*	*s 48·5*
15. Workington	„	„	„
16. Percy Main.....	„	„	„
17. Killingworth.....	„	„	„
18. Cornwall	*Various copper and tin mines*	*100—500*	*50**
„	„ „	„	„
„	„ „	„	„
„	„ „	„	„
„	„ „	„	„
„	„ „	„	„
„	„ „	„	„
„	„ „	„	„
19. „ Dolcoath.....	*Copper and tin mine* ..	*280*	*50*
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
20. „ Huel Vor	*Tin mine*	..	*51*
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„

* In Cornwall a mean surface temperature of 50°, 51°, 52°, or even 53° F. was adopted in the early underground observations, and thermometric gradients were calculated on those several different scales. The more recent observations of mean annual temperature, which I have received from the Meteorological Office, give for Penzance 51·8°, Truro 52°, Falmouth 51·4°, while Plymouth is 51·3°, and the high ground of Dartmoor 45·8°. Taking the height of the mining districts to vary from 100 to 800 feet, we may take the mean annual temperature of those districts "in block" at 51°. Mr. R. Were Fox placed thermometers 3 feet underground, at a height of 300 feet above the sea-level, near Dolcoath Mine, which gave a mean annual temperature of 49·94° F.; at 300 feet near Gooland Mine, which gave 48·99°; and at 120 feet at Falmouth, where it recorded 50·67°. His opinion was

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
11	246	53·5°	Daubuisson, "Journal des Mines," vol. xxi, p. 119 (1807). (*a*) Temperature of St. Brieuc.
	412	56	
	489	58	
12	230	54	
	459	62·5	
	781	66	
13	200	48·2	De Trebra, 1805–7, "Ann. des Mines," vol. i, p. 377 (1816), and vol. iii, p. 59. Obs. made in glazed niches in rock. The mean of 2 years' obs. No working going on.
	558	55	
	886	59	
	1246	66	
14	480	60	R. Bald, "Phil. Jour.," vol. i, p. 135 (1819). 48·5° is the temperature of Cockermouth.
15	504	60	
16	900	68	
17	1200	74	
18	500—550	65	Dr. Forbes, "Temperature of Mines," "Trans. Roy. Soc. Cornwall," vol. ii, p. 159 (1820). Average of observations made in six mines. Gives the temperature of the air and water. These are the water temperatures. (*a*) Here there was a strong current of air.
	600—650	63	
	700—750	65	
	800—850	66	
	900—950	71 (*a*)	
	1150—1260	71	
	1260—1350	74	
	1350—1400	79	
19	240—300	58	R. W. Fox, "Trans. Roy. Soc. Cornwall," vol. ii, p. 19 (1820). (*a*) Here there were strong currents of air. Obs. in rock, except the last, which was in water.
	540—600	59 (*a*)	
	720—780	63	
	1140—1200	64 (*a*)	
	1320—1380	78†	
	1380—1440	82	
20	6—60	52	*Ibid.* All these are water temperatures. (*a*) Here the temperature of the air in gallery was 72°.
	180—240	61	
	480—540	63	
	600—660	64	
	660—720	66	
	720—780	70	
	780—840	69 (*a*)	

that the mean surface temperature of Cornwall was under 51°, and possibly even less than 50°. This will account for the apparent discrepancy between the gradients of many of the original observers and those given in the Tables II, III, IV.

† A subsequent observation (No. 73) of Mr. Fox, made a year later, at the depth of 1380 feet, gave a rather lower reading. A thermometer 4 feet long was placed in a hole 3 feet deep, at a spot where no workmen were employed, and where the current of air was small. The hole was filled with clay round the stem of thermometer, which was left in that situation for eighteen months, and was found always to indicate a temperature of 76° or 76¼°. In the experiment of 1820 the thermometer was buried in the rock to the depth of 6 or 8 inches, and filled round with earth.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
21. Cornwall, Huel Damsel ..	*Copper mine*.....	..	*51°*
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
22. Neath, South Wales* ..	*Coal pit*	*150* ?	*s 50*
23. Carmeaux (Tarn)	„	*820*	*55* ?
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
24. Littry, Calvados	„	*197*	*51*†
25. Decise, Nièvre	„	*492*	*55* ?
„ „	„	„	„
„ „	„	„	„
26. Cornwall, Huel Alfred ...	*Copper mine*.....	..	*50*
27. „ Dolcoath.....	*Copper and tin mine* ...	*280*	*50*
28. „ Huel Trumpet .	*Tin mine*	..	*50*
29. Pregny, near Geneva	*Artesian boring (not overflowing)*	*308* feet above Lake of Geneva	*48*‡
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
30. Sunderland	*Coal pit*	*87*	*47·5*
31. Tours.....	*Artesian well*	*180*	*53* ? *(a)*
32. Lille (St. Venant).....	„ „	*79*	*a 50·5*
33. Aire, Pas de Calais.....	„ „	*50* ?	*49·7*?
34. Neu Salzwerk(*a*). Westphalia.	*Artesian salt-well*	*270*	*m 48* (*b*)
Neu Salzwerk	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
35. Sheerness (*c*)	*Artesian well*	*10*	*m 49·2*
36. Paris, École Militaire....	„ „	*213*	*a 51*
37. Paris, Grenelle Well	„ „	„	*a 51*††
„ „ „	„ „	„	„
„ „ „	„ „	„	„

* The thermometer was here buried for some hours 1 to 2 feet under the ground.
† Temp. of Rouen is 50·7°.
‡ Temp. of Geneva, 48·4°; height above sea, 1335 feet.
§ French feet.
‖ Observation made in hole filled with water.
¶ The temperature of water at outflow in the first three observations did not

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
21	300—360	61°	
	480—540	69	
	600—660	70	
	720—780	70	*Ibid.* All these are air temperatures.
	780—840	73	
	840—900	70	
22	540 (*a*)	62	(*a*) Recorded by J. T. Price.
23	20	55	
	38	55·5	Cordier, "Essai sur la Température de l'Intérieur de la Terre," 1822. Temp. of the air in galleries at Carmeaux 23·5° C., and at Littry 21° C.
	597	63	
	630	67	
24	325 (*a*)	61	(*a*) The mean surface temp. was estimated from shallow wells adjoining the coal pits.
25	29	54·5	
	351	64	
	561	72	
26	930	70	R. W. Fox, "Trans Roy. Soc. Cornwall," vol. iii, p. 313, (1828).
27	1440	82 (*a*)	(*a*) Air 80.°
28	768	65	
29	100§	51·6	
	200	53·2	De la Rive and Marcet, "Mém. Soc. Phys. Geneva," vol. vi, p. 503 (1833). Thermometer protected against pressure. This depth is equal to 713 English feet.
	400	57·4	
	600	61·5	
	650	62·7	
30	1584	72·6‖	Phillips, "Phil. Mag.," vol. vi, p. 446 (1834).
31	460	63·5	Arago, "Notices Scientifiques," vol. iii, p. 347, *et seq.*
32	329	57·2	(*a*) Overflowing spring.
33	205	55·9	Arago, "Not. Scient.," vol. iii, p. 347 *et seq.*
34	787	70·7¶	(*a*) Recorded by Humboldt.
	1033	73	(*b*) Temp. of Boehum, which is one degree further south, is 48·6°. The boring was ultimately carried to a depth of 2113 feet.
	1073	81·5	
	2038	88·3	
35	361	60	(*c*) No reference given.
36	568	61·5	Walferdin, "Comptes rendus," 1836, p. 314.**
37	568	..	Walferdin, "Comptes rendus," 1837, p. 977; and Arago.
	1312	74·7	
	1656	79·6	

agree with the temperatures at depth (which are those given here) owing probably to the influx of water at intermediate depths.

** All Walferdin's observations were made with overflow thermometers protected against pressure.

†† Temp. of Paris. Another datum line of invariable temperature (53° F.) at the depth of 28 mètres (92 feet) in the cellars of the Paris Observatory, is sometimes taken.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
37. Paris, Grenelle Well	*Artesian well*	*213*	*a 51°*
38. Cornwall, Levant	*Copper and tin mine* ..	*80*	*50*
39. „ Tresavean	*Copper mine*.....	*362*	*50*
40. „ Consolidated ..	„ „	*318*	*50*
41. Rudersdorf, Berlin.....	*Artesian well ?*.....	*153*	*m 48·3*
42. St. Sever, Rouen.....	*Artesian boring*.....	*128*	*m 50·7*
43. St. André (Eure)	*Artesian well*	..	*51*
„ „	„ „	..	„
44. Yakoutsk, Siberia.....	*Well*.....	*525*	*m 13*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
45. Scotland, Carse of Falkirk.	*Artesian well*	..	*s 46·5*
„ „	„ „	..	„
„ „	„ „	..	„
„ Midlothian....	„ „	..	„
46. Cosseigne-les-Luxembourg	*Artesian boring*	..	*47*
47. Cornwall	*Various tin and copper mines*	..	*50*
„	„	„	„
„	„ „	„	„
48. Troyes, Aube	*Artesian well*	*359*	*a 52·3*
48*a*. St. Ouen, Paris.....	„ „	?	*51*
49. Alfort, Marne	„ „	..	*51·5*
49*a*. Cornwall, East Wheal Crofty.*	*Copper mine*	..	*50*
49*b*. Cornwall, Consolidated ..	„ „	*318*	„
„ „	„ „	..	„
49*c*. „ United Mines..	„ „	..	„
„ „	„ „	..	„
49*d*. „ Great Wheal Fortune.	„ „	..	*51*
49*e*. Cornwall, Marazion	*Copper and tin mine*...	..	„
„ „	„ „ ...	..	„
„ „	„ „ ...	..	„
49*f*. „ Wheal Trenwith	*Copper mine*	..	„
„ „	„ „	..	„
49*g*. „ South Roskear, Cambourne.	„ „	..	*50*
„ „	„ „	..	„

* Nos. 49*a* to 49*p* are from Mr. Henwood's paper, "Trans. Roy. Geol. Soc. Cornwall," vol. v, pp. 389—402.

Arago gives a depth of 377 feet with a temp. of 31° F.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
37	1797	81·9°	Walferdin, "Comptes rendus," 1837, p. 977; and Arago.
38	1380	80	Henwood, "Report Brit. Assoc.," 1837. Thermometer buried in rock.
39	1572	82	
40	1740	85·3	
41	880	74·3	Bischof, "Edin. New Phil. Mag.," vol. xxiv, p. 132 (1838).
42	600	63·7†	Girardin, "Comptes rendus," 1838, p. 507.
43	246	54·5	Walferdin, "Comptes rendus," 1838, p. 503.
	830	64·4	
44	50	18·5	Erman, "Comptes rendus," 1838, p. 501.
	77	19·6	
	119	23	
	182**	31	
45	231	51·5	R. Paterson, "Edin. Phil. Mag.," vol. xxvii, p. 71 (1839).
	270	51·5	
	380?	53	
	..	..	Only the rate of increase given.
46	1105	78	Biver, "Comptes rendus," vol. x, p. 41 (1840).
47	354	..	Fox, "Brit. Assoc. Rep.," p. 310 (1840). Average of 53 mines. Only the rate of increase is given. In these cases see Table III.
	438	..	
	684	..	
48	410‡	60	Walferdin, "Bull. Soc. Géol. France," vol. xi, p. 29 (1840).
48*a*	216	55·3	Arago, "Notices Scientifiques."
49	177§	57·2	Lassaigne, "Comptes rendus," October, 1842.
49*a*	480	61	Small stream from lode.
	810	70·7	Small stream from rock.
49*b*	1704	89	Moderate stream from lode-end.
	1764	92·5	Large stream from lode-end.
49*c*	1080	74	Very large stream from *cross-course*.
	1260	89·5	Moderate stream from lode-end.
49*d*	804	70	Large stream from rock.
	864	73	
49*e*	222	56·5	Moderate stream from lode-end.
	480	63	Large „ „ „
	600	66	Small „ „ „
49*f*	180	55·5	Small stream from lode.
	660	66	Large „ „ „
49*g*	702	62	Moderate „ „ „
	774	68	Small „ „ „
	834	71	

† Thermometer not protected, but pressure allowed for; remained down 16 hours.
‡ The last 107 feet were obstructed, so that the actual depth was 517 feet.
§ Water overflows.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
49*h*. Cornwall, North Roskear.	*Copper mine*	..	*50°*
„ „	„ „	..	„
„ „	„ „	..	„
„ „	„ „	..	„
49*i*. „ East Pool	„ „	..	„
„ „	„ „	..	„
49*j*. „ Wheal Uny, Redruth.	*Tin mine*	..	„
„ „	„	..	„
49*k*. Cornwall, Chacewater, Redruth.	„	..	„
„ „	„	..	„
49*l*. Cornwall, East Wheal Virgin Consolidated Mines, Redruth.	*Copper mine*	..	„
„ „	„ „	..	„
49*m*. Cornwall, Wheal Towan, St. Agnes.	„ „	..	„
„ „	„ „	..	„
49*n*. „ Wheal Prudence	„ „	..	„
49*p*. „ Wheal Vor ...	*Copper and tin mine* ...	..	*51*
„ „	„ „ ..	..	„
50. Cornwall, Binner Downs .	*Copper mine*	..	*50*
„	„	..	„
„	„	..	„
„	„	..	„
„	„	..	„
51. Devonshire, Wheal Friendship.	„	..	„
„	„	..	„
„	„	..	„
„	„	..	„
52. Cornwall, St. Ives Consols	*Copper and tin mine* ...	..	*51*
„	„ ...	..	„
„	„ ...	..	„
„	„ ...	..	„
53. Cornwall, Wheal Wreath.	*Tin mine*	..	„
„	„	..	„
„	„	..	„
„	„	..	„
„	„	..	„
54. Cornwall and Devon.....	*Various tin and copper mines*	..	*50*
„ „	„ „ ..	..	„
„ „	„ „ ..	..	„
„ „	„ „ ..	..	„
„ „	„ „ ..	..	„
55. Monte Massi, Tuscany* ..	*Shaft* (?)	*174*	*56* ?
„ „ „ ...	„	„	„

* No water. Shaft well ventilated. An abnormal centre of heat in this district.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
49*h*	402	61°	
	642	66	Small stream from lode.
	786	60·7	
	822	73	
49*i*	372	59	Large „ „ „
	„	58·5	Large stream from cross-course.
49*j*	390	58·3	Large stream from lode.
	432	60	Large stream from rock.
	486	61·5	Small stream from lode.
49*k*	768	75	Large „ „ „
	„	68	Small „ „ „
	„	72	Small stream from rock.
49*l*	1500	86·5	Large stream from lode-end.
	1722	94·5	Large stream from rock.
	„	92	Small stream from vein.
	„	91	Hole in rock.
49*m*	648	62	Moderate stream from lode.
	804	70	Small stream from rock.
	924	72	Small stream from lode.
49*n*	654	65·5	Large stream from lode.
49*p*	1420	80·5	Temperature of rock.
	1706	91	
50	300	56·5	
	756	67	
	816	65	
	936	74·5	Henwood, "Trans. Roy. Geol. Soc. Cornwall," vol. v, p. 389 and 402 (1843). The observations in all these mines were taken in springs issuing from the lode or rock.
	1056	82	
51	282	55	
	450	54	
	690	64	
	810	69·5	
52	108	57	
	642	60·5	
	462	65	
	810	71	
53	162	53	*Ibid.*, p. 387.
	1242	70	
	1422	71·5	
	„	75	
	1482	76	
54	180	54·8	
	432	60·8	*Ibid.*, p. 402. Average of various mines in ten districts.
	762	67·4	
	1050	78·6	
	1440	85·5	
55	1122	103	Matteucci, "Comptes rendus," 1843, p. 937; 1845, p. 816.
	1214	107	

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
56. Neuffen, Wurtemberg* ..	*Shaft and bore-hole*....	*1378*	*46°* ?
57. Mondorff,† Luxembourg .	*Artesian well*	*672*	*47* ?
„	„ „	„	„
„	„ „	..	„
58. Mons (Couchant de Flénu) §	*Coal pit*	..	..
59. Eastern Virginia, U.S., Mills's Pit.‖	*Coal pits*	..	*56·7*
60. Eastern Virginia, U.S., Wills's Pit.	„ „	..	„
„ „	„ „	..	„
61. Eastern Virginia, U.S., Midlothian Pit.	„ „	..	„
„ „	„ „	..	„
„ „	„ „	..	„
62. Meaux, Marne	*Artesian well*.....	..	*51*
63. Ostend, Belgium.....	„ „	*20*	*m 50*
64. Vienna.....	„ „	*637*	*m 50*
65. Mondorff, Luxembourg..	„ „	*584*	*47·3* ?
„ „	„ „	„	„
66. Charleston, U.S.A.....	„ „	*20*	*m 66*
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
67. Conselica, Ferrara, Italy¶	„ „	*27*	*m 53·9*
68. Creuzot (Torcy) Saône et Loire.**	*Bore-hole*	*1017*	*48·5*
69. Creuzot (Mouillonge) ...	„ „	*1052*	„
70. 1837. Cornwall { Par Consols }	*Tin mine*	..	*51*
71. 1837 „ „	*Copper mine*	..	„
72. 1837 „ Botallock.	„ „	*40*	*51*

* The abnormal temperature in this boring is attributed by M. Daubrée to the proximity of masses of basalt of post-miocene age; and by which basalt the adjacent rocks have been altered. The surface temperature of Tubingen is 8·7° C. The temperature from 30 mètres downwards marked 38·7° C.

† The water overflowed from a spring met with at a depth of 450 mètres. Bore-hole continued to further depth of 730 mètres. Thermometer not protected.

‡ Temperature of the water which overflowed at this level.

§ Temp. not given: only the rate of increase of 1° C. in 33·25 mètres.

‖ At Mills's and Wills's pits the temperature given is that of the water collected at bottom of shafts.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
56	1263	101°	Daubrée, "Comptes rendus," 1845.
57	1476	‡74·7	Rivot, "Ann. des Mines," vol. viii, p. 79 (1845).
	2200	93·2	
	2297	? §	
58	..	..	"L'Institut," April, 1845.
59	420	63	
60	386	62	
	570	65·5	Professor H. D. Rogers, 1846. Observations in Report on Coal Mines of Eastern Virginia.
61	330	61·7	
	600	66·2	
	780	68·7	
62	230	57·2	D'Archiac, "Histoire des Progrès de la Géologie," vol. i, p. 77 (1847).
63	967	71·6	Dewalque, "Bull. Géol. Soc. de France," vol. xx, p. 235 (1849).
64	616	60·8	"Bull. Géol. Soc. de France."
65	1647	(*a*) 78·3	Walferdin, "Comptes rendus," 1853, p. 250. (*a*) Temp. of first overflowing spring. (*b*) „ of the mud in bore-hole; this is unreliable on account of convection currents.
	2362	(*b*) 81·7	
66	50	68	Hume, "Edin. New. Phil. Journ.," vol. lvii, p. 178 (1854). Nothing said about protection against pressure or convection currents. Observations were taken at every 100 feet.
	100	68	
	400	72	
	1000	84	
	1106	88	
67	164	59	Scarabelli, "Bull. Soc. Géol. France," vol. xiv, p. 102 (1856).
68	1817	81	Walferdin, "Comptes rendus," 1857, p. 971.
69	2677	100	
70	768	74	R. Were Fox, "Brit. Assoc. Reports for 1857," p. 96.†† (*a*) Under the sea—gallery quite dry.
71	1248	84	
72	1128	(*a*) 79	

¶ Water rose 2 mètres above surface.

** Eighteen protected thermometers used. The boring at Torcy had been suspended for six months. The observations at Mouillonge were made only after one to three days' rest. The two borings are 1500 mètres apart.

†† Most of these experiments were made at or near the ends of the deepest levels of the mines. Casella's thermometers were used in the later experiments. They were placed in holes 15 to 20 inches deep in the rock, which were carefully closed with clay, tow, or cotton. Thermometer left in for ½ to 1 hour.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
73. 1822. Cornwall, Dolcoath	*Copper and tin mine*..	*280*	*50°*
74. 1857 „ „	„ „ ..	..	„
75. 1853 „ „	*Another lode*.....	..	„
76. 1853 „ Levant	*Copper and tin mine* ..	*80?*	*51*
76*a*. „ „ „	„ „ ..	..	„
77. 1857 „ „	„ „ ..	..	„
78. 1837 „ Tresavean	*Copper mine*.....	*362*	*50*
79. 1853 „ „	„ „	„	„
80. 1853 „ United or Fowey Consols.	„ „	..	*51*
81. 1857 „ „	„ „	..	„
82. „ „ „	„ „	..	„
83. Chili.....	*Colorado mine*.....	*3656*	..
„	„ „	„	..
„	„ „	„	..
„	„ „	„	..
„	„ „	„	..
84. Columbus, Ohio, U.S.A.*.	*Artesian well*	*834*	*m 53·3*
„ „	„ „	„	„
85. Dukinfield, 1848	*Coal shaft*	..	*48†*
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
86. Dukinfield, 1858.....	„ *A new shaft*	..	„
„ „	„	..	..
„ „	„	..	..
„ „	„	..	..
87. Rehme, Westphalia	*Artesian well*	..	*48·6‡*
88. Louisville, Kentucky§ ..	„ „	*450*	*m 55·7*
88*a*. Sahara Desert.....	„ „	..	..
89. Naples, Largo Vittoria‖..	„ „	..	*m 59·9*
90. „ Royal Palace....	„ „	..	„
91. Ben Tallah, Algeria	„ „	..	*68?***
92. Baraki, „	„ „	..	„
93. Oued-el-Halleg, „	„ „	..	„

* Walferdin's thermometers in strong iron case were used, but without protection against convection currents; left down 28 hours.

† The surface temperature of Manchester is M 48·6°. Dukinfield stands higher.

‡ This is the temperature of Boehum.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
73	1380	76°	
74	1632	73	
75	„	79·5	R. Were Fox, "Brit. Assoc. Reports for 1857," p. 96; also "Coal Commission Report," vol. ii, p. 211.
76	1530	(*a*) 74	(*a*) Near bottom of shaft.
76*a*	„	(*b*) 87	(*b*) Not far under the sea.
77	„	(*c*) 85	(*c*) Far under the sea—no working going on.
78	1572	82·5	(*d*) A copious spring of water here gave 93·5°.
79	2112	(*d*) 90·5	(*e*) A hot spring in another lode.
80	1728	93	
81	1530	..	
82	„	(*e*) 116	
83	288	64·8	Henwood, "Edin. Phil. Mag.," N.S., vol. vii, p. 147 (1858). The mine is 1500 feet deep; well ventilated. No water: rainless district. Obs. in holes in rock 2 feet deep.
	762	67·5	
	900	67	
	1362	72	
	„	74·5	
84	90	53	Wormley, "Amer. Journ. Science," 2nd Ser., vol. xxx, p. 106 (1860).
	2575	88	
85	17	51	Fairbairn: "Brit. Assoc. Reports," 1861, p. 53. Observations made in holes on side of shaft; thermometer left from half an hour to two hours. The holes all dry and mostly in shale or rock. No mention of temperature of air in the shaft, or of the depth of the holes.
	711	58	
	900	60	
	1119	64	
	1338	67	
	1734	72	
	2151	75	
86	502	58	
	924	60	
	1000?	62?	
	1401	66·5	
87	2280	88?	Fairbairn, "Brit. Assoc. Reports," 1861.
88	2086	83·5	Delesse, "Revue de Géologie," vol. i, p. 9 (1862).
88*a*	..	..	Only the rate of increase given. See Table IV.
89	909	71·6	Mallet, "Neapolitan Earthquakes," vol. ii, p. 311 (1862).
90	1460	68¶	
91′	459	76	L. Ville, "Ann. des Mines," 6th Ser., vol. v, p. 369 (1864). Overflowing wells.
92	426	77	
93	371	73·4	

§ Overflowing salt water; said to rise 52 mètres above surface.
‖ The water rose above the surface.
¶ Attributes this low temperature to the influx of water at different depths.
** The mean temperature of Algiers is 64·5°.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
94. Reggio, Italy	*Artesian well*	..	*65·5°**
95. Ghadamés, Tripoli	„ „	..	*73·4*
96. St. Petersburg	„ „	*15*	*m 38·6*
97. Messis, Algeria	„ „	..	*68?*
„ „	„ „	..	„
98. Meiahadalon, Algeria....	„ „	..	„
„ „	„ „	..	„
„ „	„ „	..	„
99. Chega, „ ...	„ „	..	„
100. Bothwell, Ontario, U.S..	„ „	..	*45?*
101. Rochefort, Charente Inf. .	„ „	..	*54·5*
102. Virac (Tarn)	„ „	*705*	*55?*
103. Montigny, Belgium	*Coal pit*	..	*51*
104.‖ Pendleton Colliery, Manchester.	„	*126*	*m 48·6*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
105. Hucknall Torkard Colliery, Nottinghamshire.	„	..	*s 48·9‡‡*
106. Annesley Colliery „	„	..	„
107. Kiveton Park Colliery „	„	..	„
108. Swanwick Colliery „	„	*460*	*48·*
109. Moira Colliery, Warwickshire.	„	..	*48·5*
110. Ruabon, North Wales ..	„	*420*	*48*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„

* Temperature of Messina.

† The rate of increase is given at 1° C. in 30 to 31 mètres. No other particulars.

‡ Water rose above surface, at rate of 700 gallons per hour, from the Corniferous Limestone.

§ Temperature of air in gallery 68° F.

‖ Nos. 104 to 122 inclusive are from the Royal Coal Commission, 1866–70, vol. ii, "Report on the Possible Depth of Working."

¶ This was in the floor; temperature in coal 70° at a distance of 500 yards from the down-brow. In the same level 1000 yards from the down brow, the temperature was 83° in the coal, and 82° in the floor.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
94	2297	†	"Revue de Géologie, vol. iii (1864).
95	394	84·2°	,, ,, ,, ,,
96	525	50·5	*Ibid.*, vol. iv (1865), the water overflows.
97	144	69·5	Degoussée et Laurent, "Revue de Géol.," vol. iv, p. 25 (1865). Discharge of water per minute, respectively 150, 1200, 15, 50, and 270 litres.
	277	71·6	
98	67	75	
	193	76	
	263	77·2	
99	138	73·4	*Ibid.*, p. 26 (1865).
100	475‡	54	Sterry Hunt, "Chem. and Geol. Essays," 1866, p. 159.
101	2812	111	Letter from Mauget and Lippmann, Paris, January, 1872. Overflowing mineral water.
102	971	84·2	"Revue de Géol.," vol. viii (1869). Overflowing well.
103	2180	73·4§	W. Warington Smyth, "Quar. Jour. Geol. Soc.," vol. xxiv, p. 81 (1868).
104	1650	74	"Coal Commission Report," vol. ii, pp. 90, 192, and 199.
	1944	77	
	2088	74¶	The work here had been open six years.
	2214	86**	Work here had been open six months.††
105	1250	70	"Coal Commission Report," vol. ii, p. 96. A new pit.
106	1425	73	,, ,, ,,
107	1200	71	,, ,, ,,
108	966	62·5	,, ,, Very wet shaft.
109	1030	..	,, ,, ,,
110	1002	60§§	*Ibid.*, p. 104. Temp. of air in gallery .. 58°
	1503	70·5	,, ,, ,, .. 58·5
	1605	73	,, ,, ,, .. 71
	1770	78	,, ,, ,, .. 71

** At 200 yards from the down-brow, the temperature was 80° in the coal and 84° in the floor. At 400 yards it was 82° in the coal and 86° in the floor. In a tunnel at the same level, the temperature of the shale was 76°; further on in fireclay it was 79°, and still further in hard rock 82°. Holes from 3 to 4 feet deep. Thermometers verified at Kew. Left 3 to 12 hours in holes perfectly dry.

†† When open one year the reading gave 84°.

‡‡ This is the mean annual temperature of Nottingham.

§§ This observation was not taken until long after the pit was sunk, and coal evidently cooled.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
111. Norley Coal Co., Wigan ..	*Coal pit*	*157*	*48°*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
112. Aberdare, Upper Duffryn* Colliery, No. 1 Station.	„	*400*	*48·5*
„ „ 2 „	„	*1220*	..
„ „ 3 „	„	*1330*	..
„ „ 4 „	„	*1540*	..
113. „ New Tredegar Colliery,† No. 1 Station.	„	*720*	*48?*
„ „ 2 „	„	*679*	..
„ „ 3 „	„	*1495*	..
„ „ 4 „	„	*1287*	..
114. „ Dowlais Colliery, Ironstone Mine.	„	*1170*	*47*
„ „	„	„	„
115. „ Cwmbach	„	*480*	*48·5*
„ „	„	*1300*	„
116. Hetton Colliery, Durham‡	„	*400*	*47§*
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„
117. South Hetton Colliery, Durham.	„	*204*	„
„ „ „	„	..	„
„ „ „	„	..	„
118. Rosebridge Colliery, Wigan	„	*157*	*48*
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„
„ „ „	„	„	„

* Beyond No. 2 station the circulation of air was partially stopped, and beyond No. 3 station entirely stopped, that part of the colliery having been abandoned for 18 months. The other Aberdare experiments were in collieries that were working at the time, and the air not shut off. The temperatures taken 4 feet deep in the coal.

† Although the surface at No. 3 station is 124 feet lower than at No. 4, it is

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
111	1049	75°	"Coal Commission Report," p. 104. Temp. in gallery.... .. 71°
	1184	75	,, ,, ,, ... 72
	1282	78	,, ,, ,, ... 73
	1487	80	,, ,, ,, ... 70
			Distance
112	360	61	*Ibid.*, p. 105, *et seq.* Temperature of air in gallery.. 62° F. from shaft. 27 yards.
	1210	65	,, ,, .. 66 1587 ,,
	1400	68	,, ,, .. 66 1877 ,,
	1690	75	,, ,, .. 74 2327 ,,
113	865	58	,, ,, .. 60 110 ,,
	920	63	,, ,, .. 70 570 ,,
	1673	69	,, ,, .. 71 2090 ,,
	1549	67	,, ,, .. 72 1370 ,,
114	371	56	,, ,, .. 55
	536	59	,, ,, .. 58
115	250	55	
	988	61	
			Distance
116	1100	60	*Ibid.*, p. 123. Temperature of air in gallery..... 50° F. from shaft. 312 yards.
	1135	68	,, ,, .. 69 1935 ,,
	1270	63	,, ,, .. 58½ 955 ,,
	1315	69·5	,, ,, .. 68 2980 ,,
	1360	66	,, ,, .. 62 1640 ,,
	1395	71	,, ,, .. 73 4332 ,,
	1400	70·5	,, ,, .. 72 3550 ,,
117	1368	72‖	
	1662	82	*Ibid.*, p. 123.
	1938	96	
118	483	64·5	
	564	66	*Ibid.*, pp. 143 and 188. All the holes were 1 yard deep and made air-tight. Holes allowed to stand 8 hours before thermometers were put in. Thermometers left 30 minutes.
	1674	78	
	1815	80	
	1890	83	

further in the heart of the mountain, and the rise of surface at No. 4 is very abrupt. Temperatures taken 4 feet deep in coal, in dry holes and no gas.

‡ Holes in coal 3 feet deep, filled with water, and left 48 hours. Thermometers then placed in them for 24 hours.

§ The mean annual temperature of Durham is 47·1°, and of Seaham is 47·7.°

‖ These temperatures were taken after boring operations had been suspended about a week; bore-holes full of water.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
Rosebridge Colliery, Wigan	*Coal pit*	157	*48°*
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
119. Sharlston Colliery, Barnsley†	,, ,, ...	*240*	,,
120. Victoria Colliery, Wakefield.	,, ,, ...	*140*	*m 48·5*
121. Worthington Colliery, Lancashire.	,, ,, ...	..	*48* ?
122. Ram's Mine, Lancashire, Pendleton Colliery.§	,, ,, ...	*175*	*48·6*
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
,, ,, ,,	,, ,, ...	,,	,,
122*a*. Cornwall, Tresavean‖ ..	*Copper and tin mine*...	..	*50*
123¶. Blythswood, nr. Glasgow	*Bore-hole*.....	?	*m 47***
,, ,, .	,, ...	..	,,
,, ,, .	,, ...	..	,,
124. South Balgray ,, .	,, ...	?	,,
,, ,, ,, .	,, ...	..	,,
,, ,, ,, .	,, ...	..	,,
,, ,, ,, .	,, ...	..	,,
125. Carrickfergus, Belfast....	*Salt pit shaft*	..	*m 48·8*††
,, ...	,, ,, ...	..	,,
126. Rosebridge, Wigan.....	*Coal shaft*.....	*157*	*s 48*
,, ,, ...	,, ...	,,	,,
,, ,, ...	,, ...	,,	,,
,, ,, ...	,, ...	,,	,,
,, ,, ...	,, ...	,,	,,
127. St. Louis, U.S. America..	*Shaft to 71 feet, then a bore-hole*	*481*	*m 55*
,, ,, ...		,,	,,
128. Mont Cenis Tunnel.....	.....	*9529*	*27·3*‡‡

* The air at this depth was 18 to 22° lower than the rock temperature.

† The holes in this and the following pit were in coal and 2 yards deep, and perfectly dry. Thermometers left two days.

‡ This was the temperature of a brackish spring issuing from a grit rock 2′ 9″ thick met with in sinking the shaft, and discharging about 1600 gallons per hour.

§ These observations were taken two years after those of Mr. Knowles; the first three were made in holes 3 feet deep.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
	1989	85°	"Coal Commission Report," pp. 143 and 149. All the holes were 1 yard deep and made air-tight. Holes allowed to stand 8 hours before thermometers were put in. Thermometers left 30 minutes.
	2037	87	
	2202	88·5	
	2235	89	
	2283	91·5	
	2322	91·5	
	2349	92 ?	
	2400	93*	
119	1005	65	*Ibid.*, p. 157. Air in gallery.. 63° Distance from shaft. 270 yds.
120	1455	78	„ „ 65 600 „
121	1803	82‡	*Ibid.*, p. 194.
122	1944	72	*Ibid.*, p. 199. At 300 yds. from engine brow, 3 feet deep.
	2088	78	At 500 yds., hole 3 feet deep.
	„	82	At 1000 yds., hole 3 feet deep.
	2214	84	At 930 yds. from brow; hole 7 ft. 9 in. deep.
122*a*	2130	99	*Ibid.*, p. 85. Dry level hole, 4 ft. deep.
123	60	47·9	Brit. Assoc. Report of 1869. Water in bore-hole.
	180	50·5	
	347	53·7	
124	60	48·2	*Ibid.* Original depth 1040 ft.; bore-hole silted up to 525 ft., and full of water.
	180	51·1	
	360	55·4	
	525	59·5	
125	570	62·4	*Ibid.* A few feet of water in both shafts.
	770	66	
126	600	66 ?	The temperatures (except the first) were taken during the sinking of the shaft by drilling a hole to the depth of a yard, plugging with clay, and leaving the thermometer ½ an hour.
	1674	78	
	1989	85	
	2235	89	
	2445	94	
127	3029	107	Brit. Assoc. Report of 1870. Better data wanted.
	3843	105	
128	5282	85·1	Brit. Assoc. Report of 1871.

‖ Coal Commission Report, A 4, vol. ii.

¶ Nos. 123 to 165*a* are from the reports of Prof. Everett, Secretary of the Committee of Underground Temperatures, in "Trans. Brit. Assoc.," 1869–83, inclusive.

** Mean annual temperature of Glasgow.

†† Mean annual temperature of Belfast.

‡‡ This is the estimated mean temperature of the surface summit level.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
129. Kentish Town	*Artesian well*	*187*	*49°*
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
„ „	„ „	„	„
130. Allanheads, Northumberland (Gin Hill Shaft)	*Lead mines*	*1360?*	*s 44·2*
„ „	„ „	..	„
„ „	„ „	..	„
131. Allanheads, Northumberland (High Engine Shaft)	„ „	..	„
132. Allanheads, Weardale, Slit Mine	„ „	..	*45·3*
133. Allanheads, Breckonhill Shaft	„ „	*1174*	*44?*
„ „	„ „	„	„
134. Crawriggs, near Glasgow.	*Bore-hole*	*200*	*47*
„ „	„	„	„
„ „	„	„	„
135. Moscow	„	*466*	*a 39·5*
„	„	„	„
136. Durham, South Hetton Colliery	*Shaft and bore-hole*....	*100?*	*47·5*
„ „ „	„ „	„	„
„ „ „	„ „	„	„
137. Paris, La Chapelle St. Denis	*Artesian well*	..	*51·5*
„ „ „	„ „	..	„
„ „ „	„ „	..	„
138. Stowmarket	„ „	*185*	*s 49·4*
„	„ „	„	„
139. Przibram, Bohemia‡	*Adalbert silver mine* ..	?	*44·7?*
„ „	„ „ ..	..	..
„ „	„ „ ..	..	..
„ „	„ „ ..	..	..
140. Seraing, Liège, Marie Colliery	*Coal pit*	*177?*	*m 51·1*
„ „ „	„	„	„
141. „ Henri Guillaume	„	„	„

* Subsequent observations made in 1879 established a temperature of 67·06° at 1008 feet.

† The thermometer could not be sunk below 857 feet, but the shaft extends to the depth of 957 ft.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
129	305	56°	These are the results* of repeated observations commenced in 1869, by Mr. G. J. Symons. Rate of increase down to 910 ft. is 56 ft. for 1° F. Below that 49 ft. for each degree.
	500	60	
	700	62·8	
	850	65	
	1000	67·8	
	1100	69·9	
130	340	49·3	Water stands in shaft at 328 ft. No reliance is placed on this determination.
	390	51·2	
	440	51·3	
131	857†	65·7	Water stands at 797 ft.
132	660	65·1	Shaft full of water.
133	42	46·5	Water stands 24 ft. down shaft. Unreliable.
	342	46·6	
134	50	47	No weight attached to this determination.
	200	50	
	350	51	
135	350	50	Same temperature at all depths.
	994	50	
136	1166*a*	66	*a*. 100 feet deep in bore-hole. Brit. Assoc. Report of 1872. Shaft is 1066 feet deep, and bore-hole 863 feet. Total depth 1924 feet.
	1466	72	
	1736	77⅛	
137	328	59·5	Brit. Assoc. Report of 1873. The diameter of this bore-hole is 4 feet. Convection currents interfere with these results.
	1312	69	
	2165	76	
138	100	53	Original depth was 895 feet. Blocked. Uncertain results.
	283	54	
139	621	50·7	Brit. Assoc. Report of 1874. Observations made in holes 2 feet deep and far from the workings. Temperature of air not given.
	1209	58·3	
	1652	61·2	
	1900	61·4	
140	761	77	Temperature of air in gallery 77½° F. Observations made in holes 5 mètres deep. Thermometer left 24 hours.
	1017	78	
141	1656	87	

‡ The section of this mine shows fifteen shafts. Herr Grimm attributes the slow increase of heat to the rocks, which are of Silurian age, being very quartzose.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
142. Chiswick, Middlesex.....	*Artesian well*	*25*	*49·6°*
„	„ „	„	..
„	„ „	„	..
143. Swinderby, Lincoln	*Bore-hole*	*120?*	*48·5?*
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
„ „	„	..	„
144. Sperenberg, Berlin	„	?	*m 48·3**
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
„ „	„	„	..
„ „	„	„	..
144*a*. St. Petersburgh	*Artesian well*	..	*39·17*
145. Anzin, Nord, France	*No.* 1 *Colliery shaft*...	..	*50·5*
„ „	„ „ ...	..	„
146. „ „	*No.* 2‡ „ „ ...	..	„
„ „	„ „ ...	..	„
147. „ „	*No.* 3 „ „ ...	..	„
„ „	„ „ ...	..	„
148. „ „	*No.* 4 „ „ ...	..	„
„ „	„ „ ...	..	..
149. Schemnitz, Hungary	*Elizabeth silver mine.* *Maximilian* „ . *Amelia* „ . *Stefan* „ . *Siglisberg* „ .	from *1633* to *2504*	from *47* to *42* (mean *44·3*)
„ „			
„ „			
„ „			
„ „			
150. Boldon, Newcastle	*Coal pit*	*97*	*47·1*§
„ „	„ „	„	„
151. Manegaon, India.....	*Bore-hole*	*1400*	*75?*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
152. Pontypridd, South Wales.	*Coal pit*‖	*554*	*49?*

* This is the mean temperature of Berlin.
† The depths here given are in Rhenish feet, 4052 Rhenish = 4172 English feet.
‡ In this shaft there was a seam of decomposing coal at a depth of 90 mètres.
§ Temperature of South Shields.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
142	65	56·2°	Brit. Assoc. Report of 1875. Water stands 60 feet from surface. 5 feet shaft down to 200 feet, then a bore-hole.
	206	55	
	395	58	
143	100	68	Brit. Assoc. Reports of 1875-6. Springs at 790 and 950 feet. Strong convection currents affect results.
	500	68¾	
	1000	69⅛	
	1300	70⅛	
	1500	73	
	1950	78	
	2000	79	
144	100†	55	Brit. Assoc. Report of 1876. Diameter of bore at 3390 feet, 12 inches; then reduced to 6 inches. Bore-hole plugged to protect against convection currents, and observations corrected for pressure.
	700	70·8	
	1100	79·5	
	1500	84·5	
	1700	87·5	
	2100	96·3	
	3390	115·5	
	4052		
144*a*	656	54	Water overflows.
145	126	56½	Obs. made in holes 0·6 to 0·7 m. deep in sides of shaft during sinking. Little circulation of air; ¼ hour elapsed between boring hole and inserting thermometer. Temp. of air in the wet shafts, 1, 2, and 3 was from 52 to 54°: in the dry shaft, No. 4, 59°.
	658	67¾	
146	286	55	
	607	63½	
147	286	56	
	472	62½	
148	69	70¾	
	442	84	
149	1368	..	Brit. Assoc. Report of 1877. The actual temperatures are not given; only the rate of increase which averages for the 5 mines 75½ ft. for each 1° Fahr.
	830		
	935		
	715		
	1358		
150	1365	75	Still air in gallery 78½°. Trial holes 10 ft. deep; left four weeks to cool, temp. falling from 81 to 79°. Air travelled 3 miles and nearly stagnant.
	1514	79	
151	10	81	The bore-hole had been 20 months at rest. Water stands to near top of tube.
	60	81	
	150	82·7	
	310	84·7	
152	855	62·7	Report of 1878. Hole in coal 4 ft. deep.

‖ The air current down the shaft amounted to between 20,000 to 30,000 cubic feet per minute. Neither the temperature of the air in gallery, nor the distance from the shaft are given. In the other parts of the mine, the air currents showed differences of 2 to 3°, according to the season of the year.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
153. Bootle, Liverpool.....	*Part shaft part bore-hole*	*40?*	*49·6°*
,, ,, ...	,, ,, ...	,,	,,
,, ,, ...	,, ,, ...	,,	,,
154. St. Gothard Tunnel	*Tunnel**.....	..	*31*
,, ,, ...	,, ...	..	temp. of crest.
,, ,, ...	,, ...	..	
,, ,, ...	,, ...	..	..
,, ,, ...	,, ...	..	..
155. Bristol, Kingswood.....	*Deep pit colliery*	*216*	*m 50*
,, ,, ...	,, ,, ...	,,	,,
,, ,, ...	,, ,, ...	,,	,,
156. ,, ,, ...	*Speedwell colliery*	*216?*	,,
,, ,, ...	,, ,, ...	,,	,,
,, ,, ...	,, ,, ...	,,	,,
158. Talargoch, Flintshire	*Lead mine, 1st pit*	*190*	*49·5*
,, ,, ...	,, ...	,,	,,
,, ,, ...	,, ...	,,	*49*
,, ,, ...	,, ...	,,	,,
,, ,, ...	,, ...	,,	,,
159. Dukinfield†.....	*Coal pit*	..	*48*
,, ...	,, ...	..	,,
,, ...	,, ...	..	,,
,, ...	,, ...	..	,,
160. Talargoch, Flintshire....	*Lead mine, 2nd pit*	*190*	*49*
161. Manchester, Ashton Moss	*Coal pit*	..	*48?*
162. Cheshire, Bredbury.....	,, ...	..	*48·6*
,, Nook Pit	,, ...	..	,,
163. Radstock, Somerset, Wells May Pit	,, ...	..	*50*
164. ,, Ludlow Pit....	,, ...	..	,,
,, ,, ...	,, ...	..	,,
165. Southampton Common ..	*Artesian well*	*140*	,,
165*a*. Ballarat, Australia.....	*Copper mine*.....	..	..
166. Pitzbuhl, Magdeburg....	*Artesian well?*.....	..	*48·5?*
167. Artern, Thuringia	,, ,, ...	..	*47·5?*

* The temperature of the springs in the tunnel was found to be higher than that of the rock.

† These additional observations by Mr. Garside were made in the coal eams in

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
153	226	52°	Bore-hole at top 24 in. in diameter. No correction for convection currents. Springs met with at depths of 318, 800, and 1303 ft.
	1004	58·1	
	1302	59	
154	3100	79·9	Reports of 1878–9. Swiss end of tunnel.
	4101	83·8	
	4615	82·8	Italian end of tunnel.
	4965	83·7	
	4108	85·1	
155	441	54·7	"Brit. Assoc. Report," 1879.
	1367	68·5	
	1769	74·7	Ventilation slight, and care taken to avoid air-currents. Trial holes 2 ft. deep, plugged, and thermometer left twelve hours.
156	1232	66·7	
	1439	69·7	
	1769	74·7	
158	465	53·4	Report of 1880.—Obs., distant from shaft 570 ft. Holes 2 feet deep.
	555	52·9	,, ,, ,, 321 ,,
	636	58·8	,, ,, ,, 2522 ,,
	660	54	,, ,, ,, 360 ,,
	1041	60·8	,, ,, ,, 570 ,,
159	1987	74	Distance from air shaft, 1380 ft.; air 71½°.
	2407	80	,, ,, ,, 1890 ,, ; ,, 78¾.
	2416	81	,, ,, ,, 1800 ,, ; ,, 79.
	2700	86·5	,, ,, ,, 480 ,, ; ,, 75½.
160	660	62	Report of 1881. 1200 ft. from shaft. Air still.
161	2790	85·3‡	Hole 3½ ft. deep.
162	1020	62	The temperature of air in the galleries in these and preceding pit are not given.
	1050	62·3	
163	560	61·7	In holes 2 ft. deep. Distance from shaft and temp. of galleries not given. A moderate current of air was passing.
164	810	63	
	1000	63	
165	1210	69·7	"Brit. Assoc. Report," 1883.
165*a*	760	72·5	Holes 3 ft. deep filled with water.
166	495	..	De Lapparent's, "Géologie," 1881, p. 372.
167	1012	..	*Ibid.* The rate of increase only given.

holes 4 feet deep, and thermometer left 48 hours. The pit was entirely free from water.

‡ The gallery was free from any strong air-current and the ground newly opened.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
168. Buda-Pesth, Hungary ..	*Artesian well*	..	*a* 50·5°
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
„ „ ..	„ „	..	„
169. Chicago	*Artesian well*	*600*	*m* 45·9
170. Chili, Chañarcillo	*Silver mine*	..	*64*
171. Brazil, Minas Giraës	*Iron mine*	..	*64·49*
172. Sark and Herm, Channel Islands	„	..	*s* 51·6
172′. Sheboyan, Wisconsin† ...	*Artesian well*	*620*	..
173. Ireland, various mines ..	*Metalliferous mine*	..	*50* ?
174. Cornwall, mean of ten mines.	„ „	*100—300*	..
175. Ireland, Wicklow	„ „	..	*s* 49·5
176. „ Waterford.....	„ „	..	*50* ?
177. „ County Cork	„ „	..	*m* 51 ?
178. Venice, Villa Francisco Grande.	*Artesian well*	..	*m* 55·6
179. Venice, Gasworks.....	„ „	..	„
180. Brussels, Belgium	„ „	*193*	*m* 50·3
181. Aerschot, „ ‡.....	„ „	..	„
182. Minden, Prussia	„ „	*238*	*m* 48§
183. Arcachon, Gironde.....	„ „	..	*55·4*
184. Pondicherry, East India .	„ „	..	*a* 82·5
185. Dundee.....	„ „	*160* ?	*m* 46·6
186. Bradford, Yorkshire.....	*Ordinary well*	*366*	*s* 47·8
187. Blackburn	*Abandoned coal shaft* ..	*347*	*48*‖
188. Birmingham private well.	*Ordinary well*	*340* ?	*m* 48·7

* An infiltration of water from a higher level was suspected.

† Flowing water was obtained at 1340 feet in the upper portion of a Silurian sandstone. Water rose 104 feet above the surface. Discharge of water = 225 gallons per minute.

	V	VI	VII
	Depths below surface.	Temperature at depths.	References and Remarks.
	Feet.	Fahr.	
168	190	59°	Communicated (1882) by Professor Judd, from letter of Professor Szabò. The observations were made in 1877 and 1878. There are hot springs and trachytic rocks in the neighbourhood. The high temperatures are dependent upon neighbouring old volcanic centres of activity.
	216	64·4	
	328	73·4	
	1279	116	
	1640	125	
	1968	142	
	2297	156	
	2487	163	
	2900	176	
	2966	178	
	3183	164	
169	751	55*	Schott in "Smithsonian Inst. Report," p. 249, 1874.
170	930	69·2	Henwood, "Trans. Roy. Soc. Cornwall," vol. viii, p. 751 (1871).
171	318	67·9	
172	222	55·5	
172′	1475	59·1	Chamberlin, "Geology of Wisconsin," vol. ii, p. 165 (1873–77).
173	342	53·4	
174	672	68·8	The observations were made in sumps or springs.
175	552	55·5	
176	672	57·5	
177	840	61·5	
178	118	58	Laurent, "Revue de Géologie," vol. xi, p. 258 (1875).
179	236	62·5	
180	215	54	Vincent and Rutot, "Ann. Soc. Géol. Belg.," vol. v, p. 77 and 99 (1878).
181	453	57·2	
182	2230	90·9	Raulin's "Géologie," 1879, p. 84.
183	413	61·9	
184	261	93·7	Medlicott, "Geol. Survey of India," 1881.
185	238	50	Sixth Report of Rivers' Pollution Commission, 1868. "Temperature of Wells."
186	360	54·5	
187	210	49·5	
188	300	50	

‡ The water rose 5 mètres above surface.
§ See temperature of Boehum, p. 60.
‖ The temperature of Stonyhurst is 47·9°.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
188′. Birmingham Waterworks.	*Ordinary well*	*350*	*m* 48·7°
189. Kidderminster	„ „	*320*	*49* ?
190. St. Helen's Waterworks .	„ „	*100* ?	*49*
191. Tranmere, Cheshire	„ „	..	*49·4**
192. Wallasey, „	„ „	..	„
193. Worksop	„ „	*127*	*m* 48·7
194. Scarborough	„ „	*176*	*m* 47·8
195. Eastbourne	*Artesian well*	*25*	*m* 50·9
196. Deal Waterworks	*Ordinary well*	*20*	*50*
197. Dover Castle.....	„ „	*380*	*m* 50·3
198. Dover Waterworks.....	„ „	*40* ?	„
199. Grimsby Docks	„ „	*10*	*48*
200. Deptford Waterworks...	„ „	*31*	*s* 50·3
201. Sittingbourne	„ „	*50*	*49·5* ?
202. Braintree.....	*Artesian well*	*220*	*49·5* ?
203. Wimbledon.....	„ „	*170*	*49·6*
204. Carisbrooke Castle, I. of W.	*Ordinary well*	..	*50* ?
205. Colchester	*Artesian well*	*109*	*m* 49·4
206. Trowbridge Waterworks	*Ordinary well*	*140*	*49·5* ?
207. Ostend†	*Artesian well*	*10* ?	*m* 50
„	„ „	..	„
„	„ „	..	„
208. Bourbourg,‡ near Dunkirk	„ „	..	*50·2*
209. Dunkirk	„ „	..	*m* 50·2
210. Newport, Isle of Wight..	„ „	*60*	*50·9(a)*
211. Gosport.....	„ „	*20*	*m* 50·6
212. Bothwell,§ Ontario	„ „	..	*45* ?
213. Croix, Dept. du Nord....	„ „	..	*50* ?
214. Mons, Grameries Colliery.	*Coal pit*	*344*	*50·5*
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„

* 49·4 is the temperature of Chester.

† The water issued at the surface with a temperature of 18° C., and rose 8 mètres above sea-level at the first depth, and 11 mètres at the last depth.

‡ The well was carried at a depth of 250 mètres into the chalk.

§ The water rose above surface at rate of 700 gallons per hour, from the Corniferous limestone.

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
188′	400	53·60°	
189	160	54	
190	270	50	
191	428	50	
192	246	51·8	
193	214	51·8	
194	214	54	
195	100	50	
196	115	52	
197	367	55·4	Sixth Report of Rivers' Pollution Commission, 1868. "Temperature of Wells." (I am responsible for the description of wells.—J. P.
198	220	52	
199	300	52·5	
200	250	54	
201	400	53	
202	430	54	
203	200	54·3	
204	240	52·5	
205	400	52·6	
206	200	52	
207	567	59	Letter from Prof. Dewalque; February, 1883.
	617		
	981	71·6	
208	544	59	Letters from Prof. Gosselet, February, 1883.
209	426	52	
210	467	62	Letter from Mr. H. Turner, September, 1883.
211	372	55	(*a*) The temperature of Osborne.
212	475	54	Sterry Hunt, "Chem. and Geol. Essays," p. 159.
213	271	57	Letter from M. Ortlieb, Feb., 1883. Average of 7 wells.
214	679	66¶	Letter from M. F. L. Cornet, April, 1883.
	1013	69	A great quantity of water flowed from the rocks
	1141	60	in this pit.
	1325	58·5	

‖ These wells pass through Tertiary strata and end in Carboniferous limestone. The water in all of them rises above the surface; one well delivers 12,000 litres per hour.

¶ Thermometer placed for not less than an hour in holes, 1 mètre deep, excavated in side of gallery.

I Locality.	II Place of observation.	III Height of surface above sea-level.	IV Mean annual temperature of surface.
		Feet.	Fahr.
215. Mons, La Louvière Colliery	*Coal pit*	*410*	*50·5°*
„ „	„	„	„
„ „	„ *another pit* ..	*351*	„
„ „	„	„	„
216. Mons, Cuesmes Colliery ..	„	*213*	„
„ „	„	„	„
„ „	„	„	„
„ „	„	„	„
„ „	„	„	..
„ „	„	*197*	..
217. North Seaton, Newcastle†	„	*−40*	*47·5*
218. Ashton Moss Colliery, Manchester	„	..	*48*
220. Dolcoath, Cornwall	*Tin mine*	*280*	*50*
„ „	„	..	..
„ „	„	..	..
„ „	„	..	..
„ „	„	..	..
„ „	„	..	..
„ „	„	..	..
221. Passy,‡ Paris	*Artesian well*	*158*	*51*
223. La Fayette, Indiana	„ „	..	..
224. Buenos Ayres	„ „	..	*62·7*
226. Croft, Whitehaven§	*Coal pit*	*72*	*48·5*
227. „ „	„	..	„
228. Lye Cross, Dudley‖	„	*822*	*47·5*
229. Denton,¶ Manchester ...	„	..	*48*
230. Richmond,** Surrey	*Artesian well*	..	*49·6*
231. „ „	„ „	..	„

* In another trial made 300 mètres from the shaft, the temperature at the same depth was found to be 70·75° F.

† These observations were made by Professor Lebour at a point under the sea, half a mile beyond water-mark, and 660 feet below O.D.

‡ Diameter of well at top, 4 feet.

The observations recorded in the preceding list (Table I) are grouped in the following Tables II, III, IV, according to class, geological structure, and geographical position:—

	V Depths below surface.	VI Temperature at depths.	VII References and Remarks.
	Feet.	Fahr.	
215	1194	76°	Letter from M. Cornet, April, 1883. Observations made 1200 mètres from the shaft in galleries; perfectly dry and not ventilated.
	1456	81	
	787	69*	*Ibid.* In dry gallery 100 mètres from shaft.
	1361	81	*Ibid.* In a new shaft without water.
216	1548	79·5	*Ibid.* At 2180 mètres from shaft.
	„	79·7	„ 2400 „ „
	„	78·5	„ 2600 „ „
	1680	82	„ 700 „ „
	„	81·5	„ 900 „ „
	1709	85	Ther. placed in a spring of salt water issuing from a bed of sandstone.
217	620	61	"Brit. Assoc. Report," 1883.
218	2880	84	Ther. left 48 hours in hole.
220	252	64	
	390	65	
	876	67·8	
	1118	65(*a*)	"Brit. Assoc. Report," 1883.
	1884	70(*a*)	(*a*) Observations considered defective.
	2124	83	
	2244	90	
221	1924	82·5	Letter from MM. Mauget and Lippmann, Jan. 1872.
223	213	55	"Rev. Géol.," vol. i, p. 9.
224	255	69·8	"Quart. Journ. Geol. Soc.," vol. xix, p. 69.
226	1140	73	
227	1250		"Brit. Assoc. Report for 1882."
228	700	57·5	
229	1370	66	
230	1176	70	Professor Judd in "Quart. Journ. Geol. Soc.," vol. xl, p. 724 (1884).
231	1337	75·5	

In galleries dry and not ventilated.

§ Hole 4 feet deep, bored upwards in roof of coal. The stations in this pit were under the sea.

‖ Hole 4 feet deep in shaly floor, under the "10 yards" coal.

¶ Situated in the valley near Dukinfield.

** Slight overflow, 4 or 5 gallons per minute.

1st. Coal Mines.
2nd. Mines other than coal.
3rd. Artesian Wells and Bore-holes.

Column 1 of these tables gives the original number of the observation in the general list of Table I.

Instead of the columns for the height and temperature of the surface, given in Table I, another giving the thermometric gradient, are substituted in the following tables.

For subjects peculiar to the separate tables, special columns are introduced in each case.

The gradients for the total depths are given in stronger type than the others.

Column VIII in Tables II and III gives the temperature of the air in the gallery in which the rock or spring temperatures are taken.

TABLE II.—COAL

In this table there is no separate column for the strata, as they all
In the few instances where they are overlaid by newer strata, the par-
Column IV gives the distance in yards of the place of observation
the face of the coal. It is, however, not often recorded.
Columns V and VI give the depth of the hole drilled for the ther-
In Column VII, the numbers in brackets show the difference of
while the rate of increase *between these depths* is given also in *italics* in
depth.

I Number in the general list, Table I.	II Name of colliery and place.	III Depth of pit or shaft.	IV Distance of station of observation from shaft.
		Feet.	Yards.
	England.		
14	Whitehaven Colliery*	480	..
15	Workington Colliery	504	..
16	Northumberland, Percy Main	900	..
17	„ Killingworth	1200	..
150	Newcastle, Boldon.....	1365	..
	„ „	1514	..
	„ „	*1365—1514*	..
217	„ North Seaton	620	..
30	Sunderland, Monkwearmouth.....	1584	..
116	Durham, Hetton	1100	*312*
	„ „	1135	*1935*

Those observations in which there are readily-apparent errors, or which are repeated more correctly at later dates, are not brought forward from Table I. Nor has it been considered necessary to repeat the fuller particulars there recorded, nor other observations, except such as bear upon the rate of increase of temperature with depth. The numbers in the first column will readily enable the reader to refer back to these details in the first general list.

The thermometric gradient is in all cases calculated upon the mean surface temperatures, and allowing for height above the sea-level.

PITS AND SHAFTS.

consist of the usual shales, sandstones, and coals of the Coal-measures.
ticulars are given in Column II or in the notes.
from the shaft, showing the distance the air has to travel before reaching

mometer, and whether placed in the *coal, rock, or water.*
temperature between the two depths given in *italics* in Column III, Column IX; the figures in thicker type refer to the gradient of the entire

V VI Position and depth of hole for thermometer.			VII VIII Temperature at depth		IX	X
Coal (C). Rock (R). Water (W).		Depth.	of coal, rock or water.	of air in gallery.	Rate of increase of depth in feet for each degree Fahr.	Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
14	W	..	60°	..	**42**	* At Whitehaven and Workington the Coal-measures are unconformably overlaid by 200 to 300 feet of Red Sandstones and Marls.
15	„	..	60	..	**44**	
16	„	..	68	*70°*	46	
17	„	..	74	*77*	**47**	
150	R	10	75	*75·5*	49	
	„	„	79	*78·5*	**47**	
	„	..	*(4·0)*	..	*37*	
217	..	..	61	..	**45**	
30	R	..	72·6	..	**62**	
116	C	3	60	*50*	85	
	„	„	68	*69*	55	

I Number in the general list, Table I.	II Name of colliery and place.	III Depth of pit or shaft.	IV Distance of station of observation from shaft.
		Feet.	Yards.
	England—continued.		
116	Durham, Hetton	1270	*955*
	,, ,,	1315	*2980*
	,, ,,	1360	*1640*
	,, ,,	1395	*4330*
	,, ,,	1400	*3550*
136	,, South Hetton*	1060	..
	,, ,,	,,	..
	,, ,,	,,	..
	,, ,,	*1166—1736*	..
	,, ,,	*1466—1736*	..
121	Lancashire, Worthington	1803	..
111	Wigan, Norley Coal Co.	1487	..
		600	..
126	,, Rosebridge†	1674	..
	,, ,,	2445	..
	,, ,,	*1674—2037*	..
	,, ,,	*2037—2445*	..
	,, ,,	*1674—2445*	..
104	Manchester, Pendleton‡ (340 feet of Triassic and Permian strata overlie the Coal-measures)	1944	..
	,, ,, ,,	2214	*400*
	,, ,, ,,	*1944—2214*	..
122	,, ,, ,,	2088	*500*
	,, ,, ,,	,,	*1000*
159	Dukinfield, Astley's‖ (708 feet of New Red Sandstone overlie the Coal-measures)	1987	*460*
	,, ,, ,,	2416	*600*

V	VI		VII	VIII	IX	X
Position and depth of hole for thermometer.			Temperature at depth		Rate of increase of depth in feet for each degree Fahr.	Notes and remarks.
Coal (C). Rock (R). Water (W.)		Depth.	of coal, rock, or water.	of air in gallery.		
		Feet.	Fahr.	Fahr.	Feet.	
116	C	3	63°	*58·5°*	79	* The temperature observations in this pit were made in a bore-hole (*a*) drilled at the bottom of the shaft, which is 1066 feet deep. The first series of observations were made by Mr. Atkinson in April, 1869 (Coal Commission Report, vol. ii, pp. 128 and 133), after the boring operations had only ceased twenty minutes. The temperature at the bottom of the bore-hole, then 858 feet deep, or 1924 below the surface, was 96°. The experiments were repeated after the boring operations had been suspended about a week, and the temperature found to be the same as before. But those made three years later (April, 1872), and recorded in the British Association Reports, show a considerable decrease of temperature. The abandoned bore-hole had then silted up to the depth of 644 feet. The encased thermometer was pushed down to 26 feet in this, or to a depth of 1736 feet from surface, where the temperature was found to be 77·1°. † The observations were made in holes at bottom of shaft during sinking. ‡ The distances in Column IV are from the down-brow. § The temperature of the rocks in this pit was from 2° to 4° higher than that of the coals. ‖ The first series of observations at Dukinfield, although taken with great care, are wanting in details. We neither know the tempera-
	„	„	69·5	*68*	59	
	„	„	66	*62*	72	
	„	„	71	*73*	58	
	„	„	70·5	*72*	**60**	
136	R	100*a*	66	..	60	
	„	400	72	..	59	
	„	670	77	..	**58**	
	„	..	(*11·1*)	..	*52*	
	„	..	(*7·1*)	..	*52*	
121	W	..	82	*62?*	**53**	
111	R	..	80	*70*	**46**	
	..	..	66	..	..	
126	R	3	78	..	56	
	„	„	94	..	**53**	
	..	..	(*9·0*)	..	*40*	
	..	..	(*7·0*)	..	*58*	
	..	..	(*16·0*)	..	*58*	
104	R§	3 to 4	77	*64*	68	
	„	„	86	*67*	59	
	..	..	(*9·0*)	..	*30*	
122	R	3	78	*65*	70	
	„	„	82	*71?*	63	
139	„	4	74	*71·5*	74	
	„	„	81	*79·0*	73	

I Number in the general list, Table I.	II Name of colliery and place.	III Depth of pit or shaft.	IV Distance of station of observation from shaft.
	England—continued.	Feet.	Yards.
159	Dukinfield, Astley's (708 feet of New Red Sandstone overlie the Coal-measures)	2700*	*160*
	„ „ „ ..	*1987—2700*	..
161	Manchester, Ashton Moss (1881)	2790	..
218	Ashton Moss (1883)	2880	..
162	Cheshire, Bredbury	1020	..
„	„ Nook.....	1050	..
119	Barnsley, Sharlston Pit	1005	*270*
120	Wakefield, Victoria Pit	1455	*600*
105	Nottinghamshire, Hucknall Torkard†	1250	..
106	„ Annesley†	1425	..
107	„ Kiveton Park†	1200	*250*
108	„ Swanwick§	966	..
109	„ Moira‖	1030	..
155	Bristol, Kingswood Pit...	441	..
	„ Deep Pit.....	1367	..
	„ „	1767	..
	„ „	*1367—1767*	..
229	Denton, Manchester.....	1317	..
156	Bristol, Speedwell¶	1232	..
	„ „	1439	..
	„ „	*1232—1439*	..
163	Bath, Radstock, Wells May Pit**	560	..
164	„ „ Ludlow Pit	810	..
	„ „ „	1000	..
110	North Wales, Ruabon.....	1002	..
	„ „	1503	..
	„ „	1605	..
	„ „	1770	..
	„ „	*1002—1770*	..
112	S. Wales, Aberdare, Upper Duffryn††.....	860‡‡	*27*
	„ „ „	1210	*1587*
	„ „ „	1400	*1877*
	„ „ „	1690	*2327*
	„ „ „	*1210—1690*	..
113	„ „ New Tredegar††	865‡‡	*110*
	„ „ „	920	*570*
	„ „ „	1673	*1370*
	„ „ „	1549	*2090*

V	VI	VII	VIII	IX	X
Position and depth of hole for thermometer.		Temperature at depth		Rate of increase of depth in feet for each degree Fahr.	Notes and remarks.
Coal (C). Rock (R). Water (W).	Depth.	of coal, rock, or water.	of air in gallery.		
	Feet.	Fahr.	Fahr.	Feet.	
159 R	4	86·5°	*75·5°*	**70**	ture of the air in the shaft nor the depth and position of the holes. Nevertheless, the later observations here given show also low temperatures. Mr. Dickinson, however, calls attention to the fact that before the shaft was sunk two of the principal seams of coal in the upper part had been worked away from the outcrop down towards the Astley shaft, and in one case a tunnel had been driven to where the shaft had come.
..	..	*(12·5)*	..	*51*	
161 R	3½	85·3	..	74	
218 „	„	84	..	80	
162 „	„	62	..	78	
„	„	62·3	..	**78**	
119 „	6	65	*63*	**63**	
120 C	„	78	*75*	**50**	
105 „	2?	70	*68·5*	60	
106 „	„	73‡	*67*	59	
107 „	„	71	*72·5?*	55	
108 „	„	62·5‡	..	72	* This pit has now (1884) been carried to the great depth of 3150 feet.
109 „	„	66	..	**61**	† These were all new pits.
155 R	2	54·7	..	83	‡ Hole at bottom of shaft.
„	„	68·5	..	77	§ Very wet pit.
„	„	74·7	..	**71**	‖ An old pit.
..	..	*(6·2)*	..	*64*	¶ The strata dip about 1 in 6.
229 ..	4	66	..	77	** Dip small. The Coal-measures in this district are covered unconformably by 100 to 200 feet of Jurassic and Triassic strata.
156 R, C	1	66·7	..	73	
„	2	69·7	..	74	
..	..	*(3·0)*	..	*69*	
163 R	2	61·7°	..	48	†† The dip is small in these Aberdare pits.
164 C	„	63	..	62	
„	„	„	..	**67**	‡‡ The depths in these pits are not the *depths of the shaft*, but are, in each pit, taken on one and the same level, and the depths given are those *beneath the surface*, the differences of depth being caused by the coal seam passing from the valley in which the shaft is situated under an adjacent hill.
110 „	3	60	*58°*	91	
„	„	70·5	*58·5*	70	
„	„	73	*71*	68	
„	„	78	„	**61**	
..	..	*(13)*	..	*59*	
112 C, R	4	61	*62*	30	
C	„	65	*65*	58	
C, R	„	68	*66*	60	
„	„	75	*74*	**54**	
..	..	*(8)*	..	*60*	
131 C	4	58	*60*	80	
„	„	63	*70*	61	
„	„	67	*72*	80	
„	„	69	*71*	74	

I Number in the general list, Table I.	II Name of colliery and place.	III Depth of pit or shaft.	IV Distance of station of observation from shaft.
	England—continued.	Feet.	Yards.
113′	South Wales, Aberdare, Vochriw Dowlais*.	1103	*66*
	„ „ „ .	1320	*466*
	„ „ „ .	628	*2000*
114	„ „ Dowlais (ironstone)	371	*160*
	„ „ „ .	536	*485*
115	„ „ Cwmbach	230	*80*
	„ „ „ .	988	*1600*
152	„ Pontypridd.....	855	..
22	„ Neath	540	..
226	Croft, Whitehaven	1140	*430*‡
227	„ „	1250	*1340*
228	Lye Cross, Dudley.....	700	..
	Belgium.		
140	Liège,‡ Seraing Collieries	761	..
	„	1017	..
141	„	1656	..
	„	*761—1656*	..
	„	*1017—1656*	..
214	Mons,§ Grameries Colliery.....	679	..
	„ „	1013	..
215	„ La Louvière Colliery.....	1194	*1312*
	„ „	1456	„
	„ „	*1194—1456*	..
	„ „ a new shaft	1361	..
216	„ Cuesmes Colliery.....	1548	*1531*
	„ „	1680	*766*
	„ „	*1548—1680*	..
	France.		
145	Anzin,‖ Valenciennes	126	..
	„ Shaft No. 1	658	..
		126—658	..
24	Littry, Calvados¶	325	*197*
25	Decise,** Nievre	561	..
23	Carmeaux,†† Tarn	630	*69*

V Position and depth of hole for thermometer.		VI	VII Temperature at depth	VIII	IX	X
Coal (C). Rock (R). Water (W).		Depth.	of coal, rock, or water.	of air in gallery.	Rate of increase of depth in feet for each degree Fahr.	Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
113′	C	4	60°	*59°*	80	* In this pit the working proceeds from a hill towards a valley.
	,,	,,	62	,,	83	
	,,	,,	61	*65*	42	
114	R	,,	56	*55*	61	
	,,	,,	59	*58*	45	† These are the distances of the stations under the sea. The sea there being about 12 fathoms deep. Taking the temperature of the sea at 48°, or deducting 72 feet, Professor Everett makes the thermometric gradients 45° and 47°, which is probably the more correct.
115	C	,,	55	*48*	36	
	,,	,,	61	*63*	78	
152	,,	,,	62·7	..	62	
22	R	2	42	..	45	
226	,,	4	73	..	47	
227	,,	,,	,,	..	51	
228	,,	,,	57·5	..	70	
140	R	16	77	*78*	30	‡ The dip of the coal in these pits is considerable. In the "Brit. Assoc. Reports," the temperature of the ground at a depth of 5 mètres, is estimated at 54° F. The gradient here given is, however, calculated on a mean surface temp. of 51°.
	,,	,,	78	*77*	40	
141	,,	,,	87	*77·5*	46	
	..	..	*(10)*	..	*90*	
	..	..	*(9)*	..	*70*	
214	R	3¼	66	..	45	
	,,	,,	69	..	56	
215	,,	,,	76	..	48	§ The dip here is also considerable, and the Coal-measures are overlaid by a thick mass (300 to 400 feet) of water-bearing Lower Cretaceous strata.
	,,	,,	81	..	49	
	..	..	*(5)*	..	*52*	
	R	3¼	81	..	45	
216	,,	,,	79·7	..	53	
	,,	,,	82	..	54	
	..	..	*(2·2)*	..	*58*	
145	R	2	56·5	..	21	|| Observations were made during sinking, in holes inserted horizontally in side of shaft.
	,,	,,	67·7	*54*	39	
	..	..	..	*(11·2)*	*47*	
24	C	2	61	*70*	36	¶ Strata nearly horizontal —pit dry.
25	,,	,,	72	*73*	33	** The strata here dip 25° S.W., and overlie crystalline rocks.
23	,,	,,	67	*74·3*	53	†† A new and dry pit; slow ventilation. The water of a well immediately above this pit, 38 feet deep, had a temp. of 55·5° F.

I Number in the general list, Table I.	II Name of colliery and place.	III Depth of pit or shaft.	IV Distance of station of observation from shaft.
		Feet.	Yards.
	North America.		
59	Eastern Virginia,* Mills's pit.....	420	..
60	„ Will's pit	570	..
61	„ Midlothian pit.....	780	..
	„ (Another account (*a*))	600	..
	„ „	780	..

TABLE III.—MINES

Those observations which were made in Springs issuing from the in Wells or sumps, W, and those in holes drilled in the Rock, R. an affix is made of R or L. The distance of the point of observation, at the spot is known in very few instances. The nature of the strata

The Thermometric Gradient in Column IX refers to the entire refer to the gradients for the intermediate depths (also in *italics* in depths that is given (in brackets) in Column VII.

I Number in general list, Table I.	II Name of mine and place.	III Nature of rock.	IV Depth.
			Feet.
	England.		
27	Cornwall, Dolcoath Mine*	*Granite.*	1440
73	„ „ (1822)..	„	1380
75	„ „ (1853)..	„	1632
74	„ „ (1857)..	„	1632
220	„ „	*Slate* (*Killas*).	252

V	VI	VII	VIII.	IX	X
Position and depth of hole for thermometer.		Temperature at depth		Rate of increase of depth in feet for each degree Fahr.	Notes and remarks.
Coal (C). Rock (R). Water (W).	Depth.	of coal, rock, or water.	of air in gallery.		
	Feet.	Fahr.	Fahr.	Feet.	
59 W	..	63°	..	**74**	* Temperature of water collected at bottom of pit. Prof. Lebour's later statement relating to Virginian coal-pits (*a*) seems more reliable.
60 „	..	65·5	..	65	
61 „	..	68·7	..	65	
?	..	?	..	45	
?	..	?	..	**55**	

OTHER THAN COAL.

rock or lode, are marked S, in Column V; those in water collected
Accordingly as the springs (S) are known to issue from *rock* or *lode*
or station, from the shaft and the temperature of the air in the gallery
in the following tables is given in Column III.
depths given in Column IV; but where the figures are in *italics* they
Column IV). It is only the difference of temperature between those

Number in general list, Table I.	V	VI	VII	VIII	IX	X
	Position of thermometer.		Temperature at depth		Rate of increase of depth for each degree F.	Notes and remarks.
	Water (W). Spring (S). Rock (R).	Depth of hole in rock.	Of the rock or water.	Of air in gallery.		
		Feet.	Fahr.	Fahr.	Feet.	
27	S	..	82°	*80°*	**45**	* The Slate in this mine extends to depths of about 800 to 900 feet, the deeper levels are in Granite. This, the deepest of the Cornish mines, has now (1884) reached a depth of 2400 feet.
73	R	2	76	..	53	
75	„	2	79·5	*78*	55	
74	„	..	73	..	71	
220	„	..	64	..	18	

I Number in general list, Table I.	II Name of mine and place.	III Nature of rock.	IV Depth.
	England—continued.		Feet.
220	Cornwall, Dolcoath Mine	*Granite.*	876
,,	,, ,, ...	,,	2124
,,	,, ,, ...	,,	2244
,,	,, ,, ...	..	*876—2124*
,,	,, ,, ...	..	*876—2244*
19	,, ,, ...	*Slate.*	240—300
,,	,, ,, ...	*Granite.*	1320—1380*
,,	,, ,, ...	..	1380—1440
122*a*	,, Tresavean	..	2130
20	,, Huel Vor.....	*Slate.*	480—540
	,, ,, ...	,,	780—840
21	,, Huel Damsel	*Granite.*	300—360
	,, ,, ...	,,	840—900
72	,, Botallock*	*Granite and hornblendic rocks.*	1128
49*f*	,, Wheal Trenwith	*Slate.*	180
	,, ,, ...	,,	660
49*g*	,, South Roskear, Cambourne.†	,,	702
	,, ,, ,, ,,	,,	834
49*h*	,, North Roskear ,,	,,	822
49*i*	,, East Pool ,,	,,	373
49*j*	,, Wheal Uny, Redruth‡ ..	,,	486
49*k*	,, Chacewater, ,, ...	,,	768
49*l*	,, East Wheal Virgin Consolidated Mines.	,,	1500
	,, ,, ,,	,,	1722
49*m*	,, Wheal Towan, St. Agnes.	,,	924
49*n*	,, Wheal Prudence ,, .	,,	654
49*a*	,, East Wheal Crofty	,,	480
	,, ,, ...	,,	810
49*b*	,, Consolidated	,,	1704
	,, ,, ...	,,	1764
49*c*	,, United Mines.....	,,	1080
	,, ,, ...	,,	1260
49*d*	,, Great Wheal Fortune ..	,,	840
	,, ,, ..	,,	864

Number in general list, Table I.	V Position of thermometer. Water (W). Spring (S). Rock (R).	VI Position of thermometer. Depth of hole in rock.	VII Temperature depth. Of the rock or water.	VIII Temperature depth. Of air in gallery.	IX Rate of increase of depth for each degree F.	X Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
220	R	3?	67·8°	..	49	
,,	,,	..	83	..	**64**	
,,	,,	..	90	..	56	
,,	,,	..	(*15·2*)	..	*89*	
,,	..	..	(*22·2*)	..	*53*	
19	R	3	58	..	34	
,,	,,	3	78	..	48	
,,	W	..	82	..	**44**	
122*a*	R	..	99	..	43·5	* In the list given in page 34, the mean between these two depths (1350 ft.) is taken.
20	W	..	63	..	42	
	,,	..	69	*72°*	45	
21	air	..	..	*61*	33	
	,,	..	..	*70*	46	
72	R	1½	79	..	40	† One level extends about 2000 feet under the sea.
49*f*	S	..	55·5	..	40	
	,,	..	66	..	44	
49*g*	,,	..	62	..	59	
	,,	..	71	..	40	‡ Thermal springs have been met with in several of the mines in this district.
49*h*	,,	..	73	..	36	
49*i*	,,	..	59	..	41	
49*j*	,,	..	61·5	..	48	§ The great Gwennap adits used to drain many of the mines in this district.
49*k*	,,	..	72	..	35	
49*l*	,,	..	86·5	..	40	
	,,	..	92	..	39	
49*m*	,,	..	72	..	42	
49*n*	,,	..	65·5	..	42	
49*a*	,,	..	61	..	44	
	,,	..	70·7	..	39	
49*b*	,,	..	89	..	44·5	
	,,	..	92·5	..	41·5	
49*c*	,,	..	74	..	45	
	,,	..	89·5	..	32	
49*d*	,,	..	70	..	44	
	,,	..	73	..	39	

I Number in general list, Table I.	II Name of mine and place.	III Nature of rock.	IV Depth.
			Feet.
	England—continued.		
49*e*	Cornwall, Marazion.....	*Slate.*	222
	„ „	„	480
	„ „	„	600
70	„ Par Consols.....	„	768
71	„ „	„	1248
51	Devonshire Wheal Friendship	„	282
	„ „	„	810
	„ „	„	*282—810*
26	Cornwall, Huel Alfred	„	930
28	„ „ Trumpet	*Granite.*	768
38	„ Levant	*Slate and granite.*	1380
76	„ „	„	1530*
77	„ „	„	1530
76*a*	„ „	„	1530
40	„ Consolidated	*Granite.*	1740
39	„ Tresavean	„	1572
78	„ „	„	1572
79	„ „	„	2112
	„ „	„	2112
50	„ Binner Downs.....	*Slate.*	300
	„ „	„	756
	„ „	„	1056
	„ „	..	*300—1056*
80	„ United or Fowey Consols	*Slate.*	1728
82	„ „ „	..	1530
52	„ St. Ives Consols†	*Granite.*	108
	„ „ „	„	462
	„ „ „	„	810
	„ „ „	„	*462—810*
	„ Wheal Wreath, St. Ives.	„	162
53	„ „ „	„	1242
	„ „ „	„	1482
	„ „ „	..	*162—1242*
	„ „ „	..	*1242—1482*
	„ „ „	..	*162—1482*
18	„ Average of six mines....	*Granite and slate.*	500—550

Number in general list, Table I.	V Position of thermometer. Water (W). Spring (S.) Rock (R).	VI Position of thermometer. Depth of hole in rock.	VII Temperature at depth. Of the rock or water.	VIII Temperature at depth. Of air in gallery.	IX Rate of increase of depth for each degree F.	X Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
49*e*	S	..	56·3°	..	43	
	,,	..	63	..	40	
	,,	..	66	..	40	
70	R	..	74	..	34	
71	,,	..	84	..	38	
51	S	..	55	..	56	
	,,	..	69·5	..	41·5	
	..	..	(*14·5*)	..	*36*	
26	S	..	70	..	47	
28	,,	..	65	..	51	
38	R	..	80	..	46	
76	,,	..	74	..	67	* The station here was near the bottom of shaft.
77	,,	..	85	..	45	
76*a*	,,	..	87	..	42·5	
40	,,	..	85·3	..	49	
39	,,	..	82	..	49	
78	,,	..	82·5	..	48·5	
79	,,	..	90·5	..	52	
	S	..	93·5	..	48	
50	,,	..	56·5	..	46	
	,,	..	67	..	44	
	,,	..	82	..	33	
	..	..	(*25·5*)	..	*30*	
80	..	..	93	..	41	
82	..	..	116	..	28	† The levels of many of the mines in this district extend beneath the sea.
52	S	..	57	..	18	
	,,	..	60·5	..	49	
	,,	..	71	..	41	
	..	..	(*10·5*)	..	*33*	
	S	..	53	..	81	
53	S L	..	70	..	65	
	S R	..	76	..	59	
	..	..	(*17*)	..	*64*	
	..	..	(*6*)	..	*40*	
	..	..	(*23*)	..	*57*	
18	W	..	65	..	35	

I Number in general list, Table I.	II Name of mine and place.	III Nature of rock.	IV Depth.
			Feet.
	England—continued.		
18	Cornwall, average of six mines	*Granite and slate.*	700—750
	,, ,, ,, ...	,,	900—950
	,, ,, ,, ...	,,	1350—1400
54	,, and Devon.* Various mines in ten districts.	,,	180
	,, ,, ,,	,,	432
	,, ,, ,,	,,	762
	,, ,, ,,	,,	1038
	,, ,, ,,	,,	1440
47	,, average of 53 mines	,,	354
	,, ,, ,, ...	..	438
	,, ,, ,, ...	..	684
130	Allanheads, Northumberland.....	*Carboniferous limestone.*	440
158	Talargoch, Flintshire	,,	660†
	,, ,, ...	,,	1041‡
	,, ,, ...	,,	636§
	,, ,, ...	..	*660—1041*
160	,, ,, ...	,,	660‖
172	Sark, Island of.....	*Syenite.*	324 384
175	Ireland, Wicklow	*Silurian schists.*	552
176	,, Waterford	*Palæozoic schists.*	672
177	,, County Cork	*Carboniferous shales.*	840
	France.		
1	Giromagny, near Belfort, Vosges ..	*Porphyritic rocks.*	332
	,, ,, ,,	..	1420
	,, ,, ,,	..	*332—1420*
12	Huelgoet, Britanny.....	*Silurian slates.*¶	230
	,, ,, ...	,,	781
	,, ,, ...	,,	*230—781*
11	Poullaouen ,, ...	,,	459
	Switzerland.		
2	Bex, near Lausanne	*Metamorphic rocks.*	721

Number in general list, Table I.	V Position of thermometer. Water (W). Spring (S). Rock (R).	VI Position of thermometer. Depth of hole in rock.	VII Temperature at depth. Of the rock or water.	VIII Temperature at depth. Of air in gallery.	IX Rate of increase of depth for each degree F.	X Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
18	W	..	65°	..	43	
	„	..	71	..	44	
	„	..	79	..	47·5	
54	..	..	54·8	..	37	* The mines in this district are many of them very old.
	..	..	66·8	..	40	
	..	..	67·4	..	44	
	..	..	78·6	..	36	
	..	..	85·5	..	40	
47	..	..	..	..	35·4	
	..	..	..	..	43·8	
	..	..	..	..	64·2	
130	W	..	57·3	..	35	
158	R	2	54	..	132	† Station 120 yards distant from shaft.
	„	„	60·8	..	88	‡ „ 190 „
	„	..	58·8	..	64	§ „ 840 „
	..	..	(6·8)	..	56	‖ „ 400 „
160	R	2	62	..	51	
172	W	..	55·5	..	81	
	S	..	57·2	..	59	
	„	..	58			
175	W	..	55·5	..	92	
176	„	..	57·4	57°	90	
177	„	..	61·5	..	80	
1	..	..	53·6	..	50	
	..	..	73	..	50	
	..	..	(19·5)	..	56	
12	W	..	54	..	75	¶ The slates are associated with quartzites and greenstone.
	„	..	66	..	49	
	„	..	(12)	..	46	
11	S	..	58·3	..	66	
2	..	..	63·5	..	30	

I Number in general list, Table I.	II Name of mine and place.	III Nature of rock.	IV Depth.
			Feet.
	Austria and Germany.		
8	Freiberg, Himmelfahrt mine.....	*Gneiss.**	870
10	„ Junghohe-Birke „	„	656
	„ „	„	1082
	„ „	„	936
13	„ Alte Hoffnung Gottes „	*Gneiss.*	200
	„ „ „ ..	„	886
	„ „ „ ..	„	1246
	„	„	*886—1246*
7	„ Beschert Gluck „ ..	*Gneiss.*	722
	„	„	984
¶			*722—984*
139	Przibram,† Bohemia	*Silurian schists.*	621
	„	..	1209
	„	..	1900
	„	..	*621—1209*
	„	..	*621—1900*
149	Schemnitz, Hungary	*Tertiary strata and syenite.‡*	1047
	America.		
3	Guanaxuato, Mexico	*Clay slate.*	1640
4	Cabrera „	?	164
83	Chili, Colorado Mine§.....	*Jurassic limestone.*	288
	„	„	900
	„	„	1362
	„	„	*900—1362*
170	„ Chanarcillo.....	„	930
171	Brazil, Minas Giräes	..	318
	„ Morre Velho	*Clay slate.*	892
	Australia.		
165*a*	Ballarat, Victoria.....	*Palæozoic rocks.*	760

Number in general list, Table I.	V Position of thermometer. Water (W). Spring (S). Rock (R.)	VI Position of thermometer. Depth of hole in rock.	VII Temperature at depth. Of the rock or water.	VIII Temperature at depth. Of air in gallery.	IX Rate of increase of depth for each degree F.	X Notes and remarks.
		Feet.	Fahr.	Fahr.	Feet.	
8	W	..	58°	*59°*	**72**	* The gneiss of this district alternates with mica-schists, and is traversed in many places by masses and dykes of granite.
10	„	..	57·2	..	50	
	„	..	62·5	..	**58**	
	„	..	61	..	55	
13	R	..	48·2	..	40	
	„	..	59	..	55	
	„	..	66	..	**54**	
	..	..	(7)	..	*51*	
7	R	..	54·5	..	63	
	„	..	60	..	**58**	
	..	..	(5·5)	..	*48*	
139	R	2	50·7			† The workings communicate with a great drainage tunnel 1360 feet above the sea-level.
	„	„	58·3			
	„	„	61·4	..	126	
	..	..	(7·6)	..	*72*	
	..	..	(10·7)	..	*120*	
149	R	1½ to 2½	..	..	74	‡ These rocks are traversed by veins of rhyolite.
3	..	..	98	..	**45**	
4	..	..	63	..	**55**	
83	R	2	64·8	*66*		§ Total depth of mine 1500 feet.
	„	„	67	*65*	**150**	
	„	„	(11·5)	*76·5*	**90**	
	..	..	74·5	..	*40*	
170	W or S	..	69·2	..	**110**	
171	„	..	67·9	..	**105**	
	..	..	81			
165*a*	R	3	72·5			

TABLE IV.—ARTESIA WELLS

Column V.—The sign + indicates that the water overflows at the surfa the hei not rise to the surface, in which case the water usually stands in a shaft sunk t little

Column VI.—These numbers give the difference between the temperature the surf differences between the special depths given in Column IV, for which the rate increas

I Number in the general list, Table I.	II Name of place.	III Nature of the strata.	IV Depth of well or boring.
			Feet.
	England.		
35	Sheerness*.....	London Clay, Woolwich Sands and Thanet Sands.....	361 450
	„		
129	Kentish Town, London...	London Clay and Sands.....325 feet... Chalk.....646 feet...	1100
	„ „ ...	Upper Greensand and Gault. 143 feet...	*0—550*
	„ „ ...	Red Sandstones (Devonian?). 188 feet†..	*550—1100*
230	Richmond, Surrey	Tertiary strata, 242, Cretaceous, 898, Oolite.....	1176
231	„ „	and Red Sandstones (Devonian?), 257	1337
142	Chiswick	London Clay, Sands, and Chalk.....	396
165	Southampton	Tertiary strata, 400, and Chalk, 810† ..	1210
210	Newport, Isle of Wight ..	Tertiary Clays and Sands.....	467
143	Swinderby, Lincoln	Lias and Rhœtic..... 140 feet.... New Red Sandstone.....1259 feet....	2000
	„ „	Permian Marls, &c..... 372 feet....	*0—1000*
	„ „	Carboniferous Shales and Limestones..... 129 feet....	*1000—2000*
153	Bootle, Liverpool.....	Trias (Red Sandstones and Marls).....	1302
	Scotland.		
45	Carse of Falkirk, Kennet House	Alluvial Beds and Coal-measures.....	270
45*a*	Midlothian, average of 11 wells.....	„ „ „	213—350
123	Blythswood, nr. Glasgow..	Boulder Clay and Coal-measures	347
	„ „ ..	„ „ „	*60—347*
124	South Balgray „ ..	„ and Greenstone	525
	„ „ ..	„ „	*120—525*
	France.		
209	Dunkirk.....	London Clay, Sands, and Chalk.....	426

WELLS AND BORINGS.

(the height of overflow is given where known); — indicates that the water does little below the water-level.
he surface and that of the depth, for which see Table I: those in brackets are ncrease is given in *italics* in Column VII.

Number in the general list, Table I.	V Rise of water relatively to the surface.	VI Difference between the temperature at surface and at depth.	VII Rate of increase of depth for 1° Fahr.	VIII Notes and remarks.
		Fahr.	Feet.	
35	—	*10·8°*	34	* For the particulars of the strata of this and the other wells in the London Basin, see Whitaker's "Mem. Geol. Survey," vol. iv, Part I, pp. 423–571.
	—	*12·8*	35	
129	*−210*	*19·7*	52·3	
	„	*(10·6)*	*52*	
	„	*(9·0)*	*60*	
230	..	*20·4*	57	
231	+	*25·9*	51·5	
142	*−60*	*8*	49	
165	—	*18·7*	65	† The lower part of both these boreholes was blocked by *débris*, the wells having been abandoned for several years before the temperature observations were made.
210	*+6*	*11·0*	43	
143	—	*30*	66	
	—	*(20)*	*50*	
	—	*(10)*	*100*	
153	—	*8·5*	153	
45	+	*5*	54	
45*a*	+	..	48	
123	—	*6·69*	52	
	..	*(5·3)*	*54*	
124	—	*12·52*	42	
	..	*(10·0)*	*40*	
209	+	*1·8*	235	

I Number in the general list, Table I.	II Name of place.	III Nature of the strata.	IV Depth of well or boring.
			Feet.
	France—continued.		
208	Bourbourg, near Dunkirk	London Clay, Sands, and Chalk*	544
213	Croix (Dept. du Nord) ...	Chalk and Carboniferous Limestone†...	271
32	Lille (St. Venant).....	Tertiary Sands and Chalk resting on Carboniferous Limestone†	329
33	Aire, near St. Omer‡	Tertiary Clays and Sands	205
36	Paris, École Militaire	Lower Tertiary Strata and Chalk	567
37	Paris, Grenelle	Lower Tertiary Strata, 148 feet.....	1797
	„ „	Chalk ..... 1394 feet.....	*0—1312*
	„ „	Clays, Greensand ... 255 feet.....	*1312—1797*
221	Paris, Passy	Same strata as at Grenelle	1924
48*a*	Paris, St. Ouen.....	Tertiary (middle) Strata	216
49	Alfort (Marne)‡	Lower Tertiary Strata.....	177
62	Meaux (Marne)‡	„ „	230
48	Troyes (Aube)	Chalk and Gault	410§
42	Rouen, St. Sever	Chalk and Jurassic Strata ?.....	600
43	St. André (Eure)	Chalk and Greensand	830
31	Tours.....	„ „	460
68	Creuzot,‖ Torcy Colliery ..	Trias, 1312 ft.; Coal-measures, 505 ft.	1817
69	Creuzot, Mouillonge „ ..	Trias, 1217 ft.; Coal-measures, 1460 ft.	2677
101	Rochefort	Triassic Beds	2812
102	Virac (Tarn)‡.....	„	971
183	Arcachon (Gironde)‡.....	Upper Tertiary Strata.....	413
	Belgium and Holland.		
180	Brussels.....	Lower Tertiary Strata	215
181	Aerschot	„	453
65	Mondorff, Luxembourg...	Lias, 177 feet; Red Marls, 675 feet.....	1647¶
	„ „ ..	Muschelkalk, 465 feet; Red Sandstone, 1020 feet; Quartzose schists, 52 feet.	2362
46	Cosseigne - les - Luxembourg**	?	1105
207	Ostend.....	London Clay and Sands, 656 feet	981
	„	Chalk, 210 feet	
	„	Red Chalk and Sand, ... 92 feet	*0—567*
		Silurian schists 30 feet?	*567—981*

Number in the general list, Table I.	V Rise of water relatively to the surface.	VI Difference between the temperature at surface and at depth.	VII Rate of increase of depth for 1° Fahr.	VIII Notes and remarks.
		Fahr.	Feet.	
208	+	*9°*	62	* The water comes from the Landenian or Thanet Sands (at 544 feet), but the bore is continued to a further depth of 275 feet in the chalk.
213	+	*7*	39	† The water rises from the Carboniferous Limestone.
32	+	*6·7*	49	‡ No information is given how these observations at these pits were made.
33	+	*5·9*	35	
36	−	*10*	54	
37	+	*30·9*	58	
	..	*(23·7)*	*55*	
	..	*(7·2)*	*61*	
221	+	*31·4*	60	
48*a*	+	*4·3*	50	
49	+?	*6*	30	
62	+?	*6*	38	
48	−	*7·8*	53	§ The lower part of the bore-hole, which was carried to a total depth of 517 feet, was obstructed. M. Walferdin supposes the heat caused by the boring instruments may have had scarcely time to be dissipated, but he assumed a higher surface temperature.
42	−	*13*	46	
43	−	*17*	59	
31	+	*10·4*	44	
68	−	*32*	57	
69	−	*51*	52	‖ At Torcy the works had been suspended six months. At Mouillonge three days.
101	+	*56·5*	50	
102	+?	*28·25*	31	
183	+?	*5·4*	71	
180	*−5*	*3·7*	51	
181	*+16*	*6·9*	65	
65	+	*31*	53	¶ The overflowing spring rose from this depth.
	..	*34·4*	68	
46	+?	..	45	** M. Biver gives the rate of increase, but not the surface temperature, nor particulars of how the observations were made.
207	+††	*21·60*	45·4	†† Although the water overflowed, it was in such small quantity that it was found necessary to take the temperatures at depths. There were small springs at 567 and 981 feet.
	..	*(9·0)*	*63*	
	..	*(12·6)*	*33*	

I Number in the general list, Table I.	II Name of place.	III Nature of the strata.	IV Depth of well or boring.
			Feet.
	Switzerland.		
29	Pregny, near Geneva.....	Molasse	713
	„	„	*0—200*
	„ „	„	*200—650*
	Italy.		
67	Conselica, Ferrara	Alluvial Beds	164
94	Reggio*	Pliocene Marls.....	2297
178	Venice, Villa Francesco ..	Alluvial Beds.....	118
179	Venice, Gas-works	„	236
89	Naples, Largo Vittoria....	Volcanic Tuffs and Tertiary Strata	909
90	„ Royal Palace.....	„ „	1460
	Germany and Austria.		
34	Neu Salzwerk	Liassic and Triassic Strata	2038
64	Vienna.....	Tertiary Strata.....	617
168	Buda-Pesth	Tertiary and Triassic Strata†	2966
			0—328
	„	„ „	*328—1640*
	„	„ „	*1640—2966*
167	Artern, Thuringia*	.. .. ..	1012
	Rudersdorff, Prussia	Triassic Strata.....	880
87	Rehme, Westphalia.....	.. .. ..	2280
144	Sperenberg, Berlin.....	Gypsum and Anhydrite, 283 feet Rhen.	4052‡
	„ „	Rock Salt ..... 3769 feet Rhen.	Rh. *0—700*
	„ „	„	*700—1500*
	„ „	„	*1500—2100*
	„ „	„	*2100—3390*
182	Minden, Westphalia	.. .. ..	2230
166	Pitzbuhl, Magdeburg* ...	.. .. ..	495
	Russia.		
144*a*	St. Petersburg§.....	Silurian Strata resting on Granite	656

Number in the general list, Table I.	V Rise of water relatively to the surface.	VI Difference between the temperature at surface and at depth.	VII Rate of increase of depth for 1° Fahr.	VIII Notes and remarks.
		Fahr.	Feet.	
29	*−20*	*14·7°*	48·5	
	..	*(5·2)*	*40*	
	..	*(9·5)*	*48*	
67	*+6½*	*5·1*	32	
94	..	..	56	* No particulars of these wells are given beyond depth and rate of increase.
178	+	*2·4*	57	
179	+	*6·9*	34	
89	+	*11·7*	78	
90	+	*8·1*	181	
34	+	*39·7*	50·5	
64	+	*10·8*	57	
168	..	*129·5*	24	† This is really a hot spring, due to the effects of old (Miocene?) volcanic action.
	..	*(26·4)*	*12*	
	..	*(51·6)*	*23*	
	..	*(53·0)*	*26*	
167	..	..	72	
	+	*26*	34	
87	+	*39·4*	58	
144	−	*67·2*	52	‡ 4052 Rhenish feet = 4172 English feet. The temp. obs. stopped at 3390 feet.
	..	*(13·0)*	*55*	
	..	*(13·2)*	*62*	
	..	*(12·1)*	*51*	
	..	*(27·7)*	*50*	
182	+	*42·3*	52	
166	..	..	48	
44*a*	+	*14·8°*	44	§ An earlier account ("Revue Géol." for 1865) gives a depth of 525 feet and a temperature of 50·5°.

I Number in the general list, Table I.	II Name of place.	III Nature of the strata.	IV Depth of well or boring.
			Feet.
	India.		
184	Pondicherry	Alluvial Beds	261
151	Manegaon	Coal-measures	310
			60—310
	Africa.†		
92	Algeria, Baraki	Marls and Gravels	426
97	„ Messis	Tertiary Strata ?	277
95	Ghadame, Tripoli	?	394
88*a*	Sahara Desert (mean of several wells)	Tertiary Marls and Gravels	..
	North America.		
66	Charleston, S. Carolina	Eocene Strata, 708 feet, overlying 398 feet of Cretaceous Strata	1106 *100—400* *400—1106*
84	Columbus, Ohio	Upper Devonian Sandstones and Shales	2575
			90—2575
88	Louisville, Kentucky	Upper Silurian Limestones and Sandstones	2086
127	St. Louis	Lower Carboniferous Shales (429) feet	3029‡
		Sandstone (360 ft.), Limestones (2725 ft.)	3843
169	Chicago§	Upper Silurian Limestone and Sandstone	751
212	Bothwell, Ontario	Corniferous Limestone (Devonian)	475
172	Sheboyan, Wisconsin	St. Peter's Sandstone (Lower Silurian)	1475
223	La Fayette, Indiana	?	213
	South America.		
224	Buenos Ayres	Pampean Beds	255

Number in the general list, Table I.	V Rise of water relatively to the surface.	VI Difference between the temperature at surface and at depth.	VII Rate of increase of depth for 1° Fahr.	Notes and remarks.
		Fahr.	Feet.	
184	..	*11·1°*	52	
151	—	*9·5**	33	* This result is assuming the mean temperature to be 75·2°, which is that of Jabalpur, a neighbouring town nearly on the same level. In the Brit. Assoc. Rept. the rate of increase (68 ft.) is calculated from the temperature at the depth of 60 feet (81°) where the water may be affected by convection currents.
	..	*(3·7)*	*68*	
92	+	*10?*	42	
97	+	*4·6?*	51	
95	+	*11*	36	
88*a*	+	..	36	† The surface temperature at the African wells is very uncertain.
	..	*22*	50	
66	..	*(4)*	*75*	
	..	*(16)*	*44*	
84	..	*35*	73	
	..	*(35)*	*71*	
88	+ *170?*	*27·8*	75	
127	—	*54*	56	‡ The observations at 3029 feet were taken with great care, and were considered reliable. The discrepancy at the greater depth is at present unaccountable, unless it were due to convection currents.
	..	*(52)*	*74*	
169	+	*9·1*	88	
212	+	*8?*	60	§ In this well an infiltration of water from a higher level was suspected.
172	+ *104*			
223	+			
224	+	*7·1*	36	

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"On the Agency of Water in Volcanic Eruptions; with some Observations on the Thickness of the Earth's Crust from a Geological Point of View; and on the Primary Cause of Volcanic Action."* By JOSEPH PRESTWICH, F.R.S., F.G.S., &c. Received March 26. Read April 16, 1885.

[PLATE 1.]

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§ 1. *Introductory Observations—Current Hypotheses—The Vapour of Water considered as the Primary Cause of Volcanic Action.*

The important part played by water in volcanic eruptions is a well recognised and established fact, but there is great difference of opinion among geologists as to whether water should be considered the primary or secondary agent, and as to the mode, time, and place of its intervention. The prevailing opinion in this country is that water is the primary cause of volcanic activity. Whichever view may be adopted, the subject is one which is so largely concerned with the laws regulating the underground circulation of water, that the consideration of the two questions must proceed *pari passu*. We shall therefore have to consider somewhat fully the hydro-geological questions relating to the circulation and penetration of water, and in connexion with this the contested question of the probable thickness of the earth's crust from the geological standpoint. The objections to the chemical theory of Davy, according to which, water finds its way to the interior of the earth, and there, meeting with the metals of the earths and alkalies, is decomposed with the evolution of intense heat, steam, and gases, have been so often stated, that it is not necessary here to refer to them further than to remark that the objections I shall have to urge generally against the percolation or passage of water to extreme depths will apply equally to this hypothesis also.

The theory of volcanic action which has of late years been most

* The general views expressed in this paper were laid before the Geological Section of the British Association at the York Meeting in 1881. See Report of Section C, p. 610.

generally accepted is that of Mr. Poulett-Scrope. As formulated by him in the successive editions of his standard work on "Volcanos,"* "the main agent in all these stupendous phenomena—the power that breaks up the solid strata of the earth's surface, raises, through one of the fissures thus occasioned, a ponderous column of liquid mineral matter to the summit of a lofty mountain, and launches thence into the air, some thousand feet higher, with repeated explosions, jets of this matter and fragments of the rocks that obstruct its efforts—consists unquestionably in the expansive force of some elastic aëriform fluid struggling to escape from the interior of a subterranean body of lava, *i.e.*, of mineral matter in a state either of fusion, or at least of liquefaction at an intense temperature. This body of lava is evidently, at such times, in igneous *ebullition*."† He further explains that the rise of lava in a volcanic vent is occasioned "by the expansion of volumes of high pressure steam generated in the interior of a mass of liquefied and heated mineral matter within or beneath the eruptive orifice," so that the vapour reaches the external "surface in a state of extreme condensation and entangled in the liquid lava which rises with and escapes outwardly, just as any other thick or viscid matter exposed to heat from beneath in a narrow-mouthed vessel *boils up* and *over* the lips of that aperture."

I might have felt some doubt as to the exact meaning of this passage, especially as the author proceeds to remark, that "at what depth those volumes of vapour are generated may be a question," but as he goes on to observe, "that the tendency to vaporisation must everywhere occasion an extreme tension" or expansive force throughout the mass, "only restrained by the enormons weight and cohesion of the superincumbent rocks," I infer from this and from the general tenour of his remarks that the steam being the original motive power, exists in and at the base of the molten magma. He says, "If any doubt should suggest itself whether this fluid is actually generated within the lava, or only rises through it, having its origin in some other substance, or in some other manner beneath, it must be dispelled by the evidence afforded in the extremely vesicular or cellular structure of very many erupted lavas, not merely near the surface, but throughout their mass, showing that the aëriform fluid in these cases certainly developed itself interstitially in every part. And although such vesicles or cells appear at first sight to be wanting in other lavas, at least in the lower portions of the lava-current after its consolidation, the microscope invariably, or almost invariably, discovers them. In those exceptional cases, where the rock is to appearance perfectly compact, it is allowable to suppose that the vapour it once contained escaped in ascending bubbles, or by exudation through

* "Volcanos." By G. Poulett-Scrope. 2nd Edit. (1862), p. 30.

† *Ibid.*, pp. 39–40.

extremely minute pores, or was condensed by pressure and refrigeration previously to the solidification of the matter."*

It must also be borne in mind that for this hypothesis to have any value, the explosive material must extend throughout the mass of lava and act from its base upwards, just as much as it is necessary that the powder in the breech of the gun should be at the back of the shot. It should therefore extend to the volcanic foci, at whatever depth that might be, and be there occluded in the lava.

Professor Judd, in his excellent summary of Mr. Scrope's views, remarks that on this hypothesis volcanic outbursts are considered to be "due to the accumulation of steam at volcanic centres, and that the tension of this imprisoned gas eventually overcomes the repressing forces which tend to its manifestation," and that "in the expansive force of great masses of imprisoned vapour, we have a competent cause for the production of fissures through which volcanic outbursts take place."†

Mr. Scrope does not enter upon the question of the mode in which the water has become occluded in the fluid magma. Sir Charles Lyell, however, in supporting the views of Mr. Scrope, says, "We may suppose that large subterranean cavities exist at the depth of some miles below the surface of the earth in which melted lava accumulates, and when water containing the usual mixture of air penetrates into these, the steam thus generated may press upon the lava and force it up the duct of a volcano, in the same manner as a column of water is driven up the pipe of a geyser."‡

Briefly the opinion of Mr. Scrope upon the cause of volcanic action is that it is to be attributed to the escape of high pressure steam generated in the interior of the earth. Before proceeding to discuss this hypothesis more fully, and to state my objections to the views of this distinguished volcanologist, I must refer to the remarkable paper of the late Mr. R. Mallet, in which the same subject is treated from an entirely different point of view.

According to Mr. Mallet, volcanic energy, as we see it on the globe, is *not the direct* product of primordial heat of fusion, although it is evidently due to the loss of that heat, and is the result of the cooling of our globe. He defines it thus: "The heat from which terrestrial volcanic energy is at present derived is produced locally within the solid shell of our globe by transformation of the mechanical work of compression or of crushing of portions of that shell, which compressions and crushings are themselves produced by the more rapid contraction, by cooling, of the hotter material of the nucleus beneath that shell, and the consequent more or less free descent of the shell by

* *Op. cit.*, pp. 36–7.

† Judd's "Volcanoes," pp. 33 and 189.

‡ "Principles of Geology," 10th Edit., vol. ii, p. 221.

gravitation, the vertical work of which is resolved into tangential pressures and motion within the thickness of the shell."*

The crushing of the earth's solid crust along lines of greatest weakness, is in this manner considered by Mr. Mallet to evolve heat sufficient to fuse portions of the crust and to cause the extrusion of the fused masses. He says, "The result of the crushing is to produce irregular masses, on the whole tending to verticality, of pulverised rock, heated more or less highly, that may extend to any depth within the solid crust; but it is only to such depths as water can percolate or infiltrate by capillarity that the deepest focus of volcanic activity can be found."

Geologists find many objections to Mr. Mallet's ingenious hypothesis. Amongst the most serious, and it seems to me fatal objections, are—(1) that it fails to explain the centralisation of the heat, for even if fully developed by sudden and paroxysmal movements, it would, unless confined to a very narrow line, which is not probable, be dispersed and dissipated throughout the whole mass affected by the pressure; (2) that there is an absence of volcanoes in the majority of mountain ranges where the pressure and crushing have been of the most powerful character; (3) while, on the contrary, in many volcanic areas there is little or no evidence of great lateral pressure, or of much disturbance of the strata beyond faulting.

The great mountain ranges of the Alps and Pyrenees, where the strata are tilted, contorted, and enormously crushed, do not contain a single volcano; the strata are highly metamorphosed, yet show no traces of igneous fusion. In the Andes, the volcanoes are mostly situated on flanking ridges, or on the lower grounds at their base, and rarely on the high central ridges. Volcanoes, in fact, are as often, or more often, on lines of fault than on anticlinal ridges. But lines of fault, even those of the greatest magnitude, show no fused walls, though the formation of slickenside surfaces must, seeing the great friction, have been attended with considerable heat.

I however entirely agree with Mr. Mallet in two of his propositions, namely, that volcanic action "is only one phase of a unique force which has always been in action . . . . since our planet was nebulous;" and in one sense "that without water we can have no volcano."†

§ 2. *Objections to Steam as the Primary Cause of Volcanic Eruptions.*

The chief objections to the hypothesis that the vapour of water is the primary agent in volcanic action, are—

1. The difficulties in the way of accounting for the presence of water in the deep-seated volcanic foci.

* "Phil. Trans.," vol. 163, p. 167.

† *Op. cit.*, p. 216.

2. The insufficiency of the elevatory force.
3. The want of agreement in *time and proportion* between the discharge of steam and the discharge of lava.

On the hypothesis of Mr. Poulett-Scrope it has to be assumed either that the water formed part of the original molten magma, or that the surface-waters find a passage by percolation or by fissures through the crust of the earth to the molten mass beneath. That water does penetrate to great depths, there can be no doubt, and if nothing interfered to check its descent, the extent and range of the percolation could hardly be limited. But various stratigraphical causes interfere with this transmission, such as the thinning out of the strata, faults, and uncomformity of superposition. These causes especially impede the flow of water in the more frequently disturbed Palæozoic strata, but in the less disturbed newer strata it is comparatively little affected by them. At the greatest depths in Tertiary and Secondary strata water is always, or almost always, met with in the permeable beds. It is otherwise with the Palæozoic strata generally; where, for example, as is so common in Coal Mines, the faulting of the measures divides them into separate segments or compartments, in each of which the supply of water from adjacent areas is cut off, and no fresh supplies being received from the surface, there is no further accession of water. Again, where Tertiary or Secondary strata overlie uncomformably Palæozoic strata, the interstices on the surface of the older rocks are so effectually plugged by the basement bed of the superimposed strata, that they are often rendered perfectly watertight. There is a remarkable instance of this in the coal-field of Mons,* where the Coal-measures are in places worked under a depth of 600 to 900 feet of loose sandy Cretaceous strata full of water, and yet are found to be perfectly dry, owing to the circumstance that the basement bed of the overlying strata has formed a sort of puddle cover, sealing up, as it were, the edges of the underlying strata. The percolation of the surface-water to great depths is, in consequence of these interruptions, far from being so general as might be supposed.

Admitting, however, the possibility of water descending in certain cases,—as through the fissures and crevices of crystalline rocks, or, in the absence of any mechanical conditions to stop its descent, through permeable strata,—it is a question whether its descent would not be stayed by the increase of heat at great depths, although under the enormous pressure it may remain liquid at high temperatures.

It is known experimentally that the pressure of steam which at 212° F. equals one atmosphere, is, at a temperature of 432° F., equal to that of nearly 24 atmospheres; and also that the rate increases with the rise of temperature, and faster at high than at low temperatures, as the following reduction from Regnault's tables shows:—

* The author in "Proc. Inst. Civil Engineers," vol. xxxvii, p. 129.

Temperature.	Pressure of mercury.	Pressure in atmospheres.	Temperature.	Pressure of mercury.	Pressure in atmospheres.
F.	inches.		F.	inches.	
212°	29·89	1·0	332°	216·21	7·21
222	36·35	1·21	342	247·38	8·25
232	43·91	1·47	352	281·99	9·40
242	52·72	1·75	362	320·28	10·68
252	62·92	2·10	372	362·50	12·08
262	74·69	2·50	382	408·92	13·63
272	88·18	2·94	392	459·80	15·33
282	103·58	3·45	402	515·41	17·18
292	121·08	4·04	412	576·02	19·20
302	140·88	4·70	422	641·90	21·40
312	163·18	5·44	432	713·32	23·88
322	188·22	6·27			

Thus, while an increase of 50° F. to the temperature of water vapour at 212° causes an increase of pressure of only $1\frac{1}{2}$ atmospheres, the addition of another 50° gives an additional pressure of nearly 3 atmospheres; of another 50° gives 5 atmospheres; while 50° more gives a further increase of $8\frac{1}{2}$ atmospheres.

This rate of increase of pressure is nearly proportionate to the 5th power of the excess of temperature above —40° F. Pouillet employed empirical formulæ to ascertain the probable pressure at higher temperatures up to 516° C. These, although not mathematically exact, are sufficiently so for our purpose—at all events up to the temperature of 773° F., the critical point of water, at which new conditions would be involved.

Temperature.	Pressure in atmospheres.
510° F.	50 atmospheres
592	100 ,,
686	200 ,,
747	300 ,,
794	400 ,,
832	500 ,,
864	600 ,,
893	700 ,,
918	800 ,,
942	900 ,,
962	1000 ,,

At the critical point of water the pressure would be nearly 350 atmospheres, or if the rule holds to the temperature at which water may undergo dissociation, we should have a pressure exceeding 1000 atmospheres. In any case these conditions point to a possible term

at which the expansive force of vapour will exceed that of the hydrostatic pressure (especially modified as it is by friction) and the descent of the surface-waters is in all probability stayed.*

Adopting the conclusion arrived in the paper I recently laid before the Society—viz., of a mean thermometric gradient of 48 feet of depth per 1° F., the following will be the relation between depth and temperature down to a depth of 150,000 feet or 28½ miles, starting with an annual mean surface temperature of 50° Fahr.

Table of Temperatures at Depths taking the Thermometric Gradient at 48 feet per 1° Fahr.

Depth.		Temperature.	
Surface	=	50° Fahr.	
500 feet	=	60½	
1,000 ,,	=	71	
1,500 ,,	=	81	
2,000 ,,	=	92	
3,000 ,,	=	112	
4,000 ,,	=	133	
5,000 ,,	=	154	
7,776 ,,	=	*212*	*Boiling point.*
10,000 ,,	=	258	
15,000 ,,	=	362	
20,000 ,,	=	467	
30,000 ,,	=	625	
34,704 ,,	=	*773*	*Critical point.*
40,000 ,,	=	883	
50,000 ,,	=	1092	
100,000 ,,	=	2133	
150,000 ,,	=	3175	

If there are experimental errors, as I consider not improbable, such as would reduce the gradient to 45 feet per degree, the temperature at the depth of 150,000 feet would be 3383°, unless the increase of heat modifies the conductivity of the rocks at depths. Or if in centres of crystalline rocks and slates, as in Cornwall, and with a gradient of 40 feet, a temperature of 3000° might be reached at a depth of 120,000 feet or about 23 miles.

* M. Delesse, in his paper on the water in the interior of the Earth, considered that notwithstanding that water tends to pass into vapour at the high temperature of great depths, the pressure of the overlying strata and the resistance they offer to its return being greater than its tension, would cause it to retain its liquid state. But at a depth which he estimates at about 60,000 feet, and at a temperature of about 1100° F., the overlying pressure (taken at the rock weight), and the elastic force of the vapour of water would be in equilibrium. "Bull. Soc. Géol. de France," 2nd Ser., vol. xix (1861), p. 64.

Besides these, there are other considerations which should not be overlooked, although it is impossible at present to assign a value to them. Still they may be placed to a suspense account. The first is whether there may not be areas of certain rocks in which the gradient is more rapid than in other areas; and whether in tropical regions there is not generally a more rapid thermometric gradient. In the paper on Underground Temperatures, a few of the observations raise these questions, as questions for further inquiry.

The second point, which I have already mooted,* is more purely hypothetical. It is whether the effect of the excessive cold of the glacial period—or cold prolonged during so many thousands of years—may not possibly have left its mark on that portion of the earth covered for so long a period by perpetual snow and ice,—whether the loss of heat in the upper layers of the crust may not only have altered the thermometric gradient, but also induced, as it were, premature contraction by an excessive abstraction of heat during that period. Whether also that outer portion of the crust so affected might not now present a slower gradient than the present mean surface temperature would warrant, while at greater depths a normal more rapid gradient may still prevail. And whether or not this might possibly be an element in the present effective rigidity of the crust?

Taking therefore into consideration all the conditions to which water becomes subject with increasing depth and the rapid increase of temperature, together with the circumstance that while the pressure of water increases with depth in simple arithmetical progression, that of the elastic vapour of water is one of a very rapid geometrical progression, it becomes extremely improbable that water can penetrate beyond a certain depth beneath the surface. Roughly, it is a question whether 7 to 8 miles would not be a limit. At all events I feel it impossible to accept any hypothesis based upon an assumed percolation to unlimited depths, and am forced to look to other causes in explanation of the presence of water in volcanic eruptions.

It is true that the experiments of Daubrée, which will be further alluded to, show that owing to the force of capillarity, water can pass through porous strata against a considerable resisting pressure, but on the other hand Wolff's experiments show that the effects of capillarity decrease with the increase of temperature, and tend to prove that there is a point at which they would altogether cease.

It may also be a question whether at the high temperature at great depths, the vapour of water would not undergo decomposition, for M. H. St. Claire Deville† has shown that under certain conditions, at a temperature of from 1103° to 1300° C., it is dissociated into its

* "Phil. Trans.," vol. 164, p. 305.

† "Sur le phénomène de la dissociation de l'Eau," "Comptes rendus," vol. lvi, p. 195.

elements, and in so dissociating it augments its volume by one half, and its pressure in proportion.* That water is decomposed in contact with lava during eruptions, is rendered probable by the observations of M. Fouqué during the last great eruption of Santorin, for he found that the gases given off under water during the eruption, and collected as they ascended through the sea, often contained as much as 30 per cent. of free hydrogen, and from the circumstance that he also found free oxygen occasionally present, he considered it likely that the vapour of water exists in a state of dissociation in the lava during eruptions.†

Other geologists have contended for the possibility of water gaining access to the volcanic foci by fissures opening into the sea-bed.‡ These fissures are supposed to be formed by the molten matter struggling to escape. To this it has been rightly objected, that in such a case the lava would at once fill the fissure to the exclusion of the water. By others it has been suggested that the fissures are caused by the escape of imprisoned elastic vapours; but as Mr. Scrope remarks, this is reasoning in a circle, for while it supposes the aqueous vapour to be the cause of the disturbance, it yet proposes to introduce the water *after* the effects attributed to it had been produced.

The second objection is, that supposing it were possible for water to penetrate to the molten magma and to be converted into high pressure steam, would it be possible for it to force forward and gradually erupt a column of lava extending from the molten mass below to the volcanic summit? Bischof's§ hypothesis was founded on an erroneous estimate of the elastic force of steam.

Would not also, on the fissure hypothesis, the pent up elastic vapours, which are supposed to force the lava up the volcanic duct, necessarily take the line of least resistance, drive back the column of water in the fissure and escape with it?

But the objection to which I attach most weight and importance is one which deals with facts which are within the scope of actual observation. On the hypothesis that attributes the extrusion of lava to "the expansive force of some elastic aeriform fluid, struggling to escape from the interior of a subterranean body of lava," it would follow that no lava could escape without the accompaniment of the propelling aeriform fluid, nor could any large evolution of vapour or gases take place without a large eruption of lava, for the relative

* At the same time it is to be observed, that enclosed in a platinum tube water does not decompose at a temperature near the fusing point of the platinum.

† "Santorin et ses Éruptions," p. 232.

‡ "Bull. Géol. Soc. de France," vol. xiii, p. 178; vol. xvi, p. 43; and 2nd Ser., vol. i, p. 23.

§ "Edin. New Phil. Jour.," vol. xxvi (1839), p. 132.

discharge of steam and lava could not fail to bear some proportion one to the other.

Although the phenomena accompanying volcanic eruptions are so constantly recorded, those which bear in particular on this question are generally so mixed up with the other details, that it is not always possible to determine their relative bearing and sequence. There are, however, an ample number of cases to show that the discharge of lava is not in proportion to the discharge of steam, nor is the discharge of steam always in accordance with the escape of lava, which they should be if the hypothesis were correct. These conditions would seem on the contrary to be perfectly independent one of the other. It is of course conceivable that lava of an extreme fluidity and offering less resistance to the escape of the elastic vapours, might be ejected in lesser quantity than a more viscid lava, which presented more resistance, or that paroxysmal explosions may disperse the lava in aerial discharges, and reduce the importance of the quieter outflow; but these occasional occurrences would not affect the more general results. There are too many great eruptions that have been attended with a small discharge of lava, and too many of the largest lava streams have been erupted quietly and with a very small exhibition of explosive violence, to allow of much doubt on the subject. Sometimes, when the discharge of lava has been at its maximum, the explosive violence has been at its minimum, and, on the other hand, violent detonations have been attended with small overflows of lava.

According to Daubeny,* there is no recorded lava-flow accompanying the eruptions of Vesuvius prior to the eruption of A.D. 1036. This, however, may be the mere absence of record. Still, it would seem to point to the prevalence of paroxysmal eruptions like that of the great eruption of 79 B.C.

Mr. Scrope divides volcanic outbreaks into periods of moderate activity and of paroxysmal violence,† and he himself remarks that "the volume of lava poured out by an eruption does not preserve any constant proportion to the force or continuance of its explosions." He instances Etna‡ as an example of almost continual moderate activity with occasionally more or less paroxysmal outbursts. The volcano of the Island of Bourbon offers another example of the same kind. He further points out§ that "in all cases where lava is emitted its protrusion marks the crisis of the eruption, which usually attains the maximum of its violence a day or two after its commencement. The stoppage of the lava in the same manner indicates the termination of the crisis, but not of the eruption, for the gaseous explosions

* "Description of Volcanos," 2nd Edit., 1848, p. 225.

† "Volcanos," 2nd Edit., p. 16—19.

‡ *Ibid.*, p. 24.

§ *Op. cit.*, p. 23.

continue often for some time with immense and scarcely diminished energy." Vesuvius "has often continued in eruption for periods of several months, discharging moderate jets of scoriæ, lapilli, and sand, from temporary orifices at the summit or flank of the cone, or at the bottom of its crater, *when there was a crater;* while streams of lava welled out, *sometimes almost with the tranquillity of a water-spring* from the same or from contiguous openings."*

Professor Palmieri† says of Vesuvius, that on some occasions the eruptions commence with explosions and detonations of greater or lesser violence, ending with a great eruption and a copious flow of lava; and that at other times great eruptions have taken place without any precursory signs.

Professor Phillips observes of the great eruption of Vesuvius of 1794, which was characterised by the flow of some of the largest lava currents ever erupted from this mountain, that "for nearly a month after the eruption (of lava), vast quantities of fine white ashes mixed with volumes of steam were thrown out from the crater."‡

M. Ch. St. Claire Deville§ states that the great eruption of Vesuvius in 1855 was *one of the most tranquil.* The projections only lasted a few days, and the detonations soon ceased. The lava continued to flow for twenty-eight days, and formed the largest current which has passed out in the north-west direction.‖ This eruption was in great contrast with that of 1850, which was one of the most violent and paroxysmal, when the mountain was changed in form, the central cone reduced, and the crater enlarged to 2 miles in circumference, *yet the flow of lava was comparatively small.*

The eruption of Etna of 1852 was one of unusual magnitude, and the flow of lava greater than ever witnessed, except probably in 1669. It commenced in August with violent explosions and ejection of scoriæ. The lava then began to flow from several openings, and flooded the country for a length of 6 miles and a breadth, in places, of 2 miles. The ejections of scoriæ continued during sixteen days, but *after that time they almost ceased,* except in a few smaller craters, though dense volumes of steam were occasionally discharged from the central crater, *but the flow of lava continued with little interruption through September, October, November, and December, and did not entirely cease until May,* 1853.¶

An eruption, which seems of itself almost sufficient to prove the

* *Op. cit.*, p. 17. The italics here and in the following pages of this chapter are mine.—J. P.

† "Eruption of Vesuvius of 1871–2." Mallet's Translation, pp. 94, 99–100.

‡ "Vesuvius," pp. 92–4.

§ "Bull. Soc. Géol. de France," 2nd Ser., vol. xii, p. 1065.

‖ "Vesuvius," p. 107.

¶ Lyell, "Phil. Trans.," vol. 148, p. 18.

independence of the causes leading to the outflow of lava, and those generating the elastic vapours, is that of Santorin in 1866, recorded by M. Fouqué.* In the centre of the bay formed by the great encircling old crater-walls of the islands of Thera and Asprosini, stands the small island of Kaimeni, the product of later eruptions. On the 26th January, 1866, the loose blocks on the southern slope of this island began to move—on the 27th slight shocks were felt, gases evolved, and fissures rent in adjacent buildings. The ground in a small sandy bay was observed to rise, and by the 4th February the erupted mass consisting of blocks of lava, had attained a height of 32 feet. By the 5th, this protuberant mass of lava had increased to 230 feet in length by 98 feet in width, and 65 feet in height, and on the 7th to $492' \times 197' \times 98'$. The adjacent water was hot and the surface of the lava was consolidated, though it was incandescent at night. Until the 12th February there were no detonations and no explosion, notwithstanding the large quantity of lava emitted, for it was not confined to the matter above water, but it was, in places, gradually filling up the bay itself; and where there previously had been soundings of 103 fathoms, the depth was now reduced to from 40 to 70 fathoms.†

M. Fouqué remarks,‡ that "at the beginning of the eruption the discharge of lava was the most salient phenomenon; *the rock-emission proceeded in silence;* it was only at the end of several days that the explosions and ejections commenced and a crater formed (the volcano of Giorgios)." The explosions attained great violence on the 20th and 22nd, and on the latter day the column of vapour and ashes rose to a height of about 7000 feet. In April and May lava flowed more freely. The eruption was prolonged to 1869, when *the explosions were still frequent but the discharge of lava very small.*

Another eruption commenced in February 1867, in the sea-bed west of Kaimeni, and by the 17th an island (Aphroessa) was formed 328 feet long by 196 feet wide and 32 feet high; while the adjacent sea-bed was in places reduced from a depth of 296 fathoms to 108 fathoms. This also was effected *quietly and without noise*, and it was not until later that the explosions began.

So noiseless and so steadily continuous was the protrusion of these masses of lava at first, that Dr. Cigalli, who watched them from day to day, compared their growth to the steady and uninterrupted growth of a soap bubble.

Much of the lava of this great eruption was very compact, and not at all scoriaceous.§

* "Santorin et ses Éruptions," Paris, 1879.

† *Ibid.*, p. 36 *et seq.*

‡ *Ibid.*, p. xv.

§ *Op. cit.*, p. 72.

But probably the eruption most remarkable for its magnitude, and at the same time for its quiet, was that of Mauna Loa in 1855. In speaking of this eruption Dana says that there *was no earthquake, no internal thunderings, no premonitions at the base of the mountain.* A small glowing point was seen at a height of 12,000 feet, which gradually expanded, throwing off coruscations of light. A vent or fissure then formed, from which a vast body of liquid lava *rapidly but quietly flowed during several weeks* (a later account says 10 months), forming a stream of lava which extended a distance of 65 miles, with a breadth of from 3 to 10 miles. He adds that those eruptions of fiery cinders which mark so strikingly Vesuvius, are almost wanting about the craters and eruptions of Mauna Loa, and the few that there are, are mainly in connexion with the lateral cones.

On the other hand, Mr. Scrope remarks that the great paroxysmal eruptions of volcanoes are preceded by earthquakes more or less violent, frequent, and prolonged, "and begin generally with one tremendous burst, which appears to shake the mountain from its foundations. Explosions of aeriform fluids, each producing a low detonation and gradually increasing in violence, succeed one another with great rapidity from the orifice of eruption, which is in most instances the central vent or crater of the mountain." As a consequence of such eruptions, the cone is frequently found truncated, "the upper part having been blown off, and in its place a vast chasm formed, of a caldron-like appearance, and of a size proportioned to the violence of the eruption and its duration.*

One of the most violent of the explosive eruptions was that of the Cosequina in 1835.† This volcano is situated on a promontory south of the Bay of Formosa in Central America. The detonations were so violent that they were heard at a distance of 280 miles. So enormous was the quantity of ashes and scoriæ shot out of the crater, that for a distance of 25 miles they covered the ground to a depth of about 15 feet, and the finer dust was carried by the wind as far as Jamaica, a distance of 800 miles. It is not recorded that this great outbreak was accompanied by any lava-flow.‡ The mountain itself is only 480 feet above the sea-level.

From time to time the violence of other paroxysmal eruptions has blown off and truncated the cone of the volcanoes, and enlarged the craters, from the small dimensions they have when the eruption issues at the finished apex, to gulfs sometimes several miles in circumference and of great depth, eviscerating, as it were, the very centre of the

* "Volcanos," 2nd Edit., pp. 20–21.

† The great eruption of Krakatoa has taken place since this was written. It was one of the same character; we wait the report now preparing by a Committee of the Royal Society.

‡ Reclus, "La Terre," p. 668.

mountain. Scrope* mentions as examples of such paroxysmal eruptions,—13 eruptions of Vesuvius, 8 of Etna, 2 of Teneriffe, 1 of San Georgis in the Azores, 3 of Palma, and 1 of Lancerote (Canary Islands), and all the recorded eruptions of Iceland.

"Sometimes in these eruptions no absolute escape of lava takes place, scoriæ alone being projected. In all cases when lava is emitted its protrusion marks the crisis of the eruption, which usually attains a maximum of its violence a day or two after its commencement. *The stopping of the lava* in the same manner indicates the termination of the crisis, but not of the eruption, *for the gaseous explosions continue often for some time with immense and scarcely diminished energy.*"†

It seems to me therefore evident from these and such other cases, that there is no definite relation between the quantity of explosive gases and vapours and the quantity of lava discharged from the volcanic foci. It is conceivable that the enormous force of some of the explosions may, in the paroxysmal outbursts, shatter and blow to fragments all the lava as it rises in the crater, but this seems hardly sufficient to account for the proportionally large quantity which should accompany such vast volumes of vapours, were those vapours the cause of the extrusion of the lava. It is still more difficult to conceive on this hypothesis the excessive discharge of lava in tranquil eruptions without a greater escape of vapour.

If the escape of lava depended altogether on the escape of the imprisoned vapours, it is not easy to see how the constant supply, whether of the lava or of the steam, is maintained. The rise and escape outwardly of the lava in a volcanic vent has been likened to the boiling up and over of any other thick and viscid matter exposed to heat from beneath in a narrow-mouthed vessel,‡ and Constant Prevost compared it to the overflow caused during fermentation by the evolution of carbonic acid gas. But the cases are not analogous. In the one the aeriform fluid is part of the substance of the vaporisable matter, which is not the case with the lava where the substance causing ejection is foreign to it. In the first case the elastic fluids are generated by a molecular change of the heated substance itself, and the supply is therefore, so long as that lasts, unlimited; whereas in the case of lava, which cannot undergo such changes, it is only the supposed occluded vapour in it, that, with the relief of pressure would be subject to expand and escape, and thereby displace and expel a proportionate quantity of the lava. Besides, could that result take place before the lava began to rise? If not, there must be an independent cause to originate the rise. We might also ask whether that very rise of

* "Volcanos," p. 25.

† *Ibid.*, p. 23.

‡ Scrope's "Volcanos," p. 40, and Lyell's "Principles," vol. ii, p. 221.

the lava in the duct would not on the contrary increase the pressure in the volcanic foci in which the occluded vapour is present?

We have already pointed out the difficulty of accounting for the introduction of water into the volcanic foci. Even supposing it to be introduced and to cause a boiling over, that ebullition would go on so long as any of the imprisoned vapour remained in the lava; but when the expulsion of one or the other was effected, then the introduction of fresh materials from the outside, as in the case of the water in the Geyser pipes, would become necessary, or the boiling over of the lava would cease for want of supplies. If the water were present in combination with the lava in the volcanic foci, there is no reason why the passage to the exterior once formed, the eruption should cease until all the mass susceptible of boiling over should be expelled, in which case each eruption would be of longer or shorter duration, and a volcano would become extinct after one eruption. If, on the other hand, the expulsion were due to the access of water from the exterior, whether by fissures or by permeation, it is difficult to imagine such an influx of water without the previous action of some disturbing cause whereby the existing equilibrium under which the descent of the water is stayed, would be destroyed.

The only logical hypothesis on which I can conceive the vapour of water of gases to be present in the fluid magma of the volcano is the one suggested by Dr. Sterry Hunt, who considers that the magma is not part of the original molten anhydrous nucleus of the earth, but an intermediate layer derived from the first outer crust of old surface rocks which had been exposed to meteorological agencies, and retained, when fused under pressure, the water with which they had become permeated when on the surface. He supposes the original nucleus to have gradually become solid by pressure and loss of heat, and an outer crust to have formed. As that crust became thicker and covered by sedimentary strata accumulated upon it, its under surface, owing to the rise of the isothermal bars, was gradually remelted, forming an intermediate fluid layer between the solid nucleus and the solid outer crust.

Or else that of Mr. Fisher, who, from investigations in which he compares the existing inequalities of the earth's surface with such as could possibly have arisen from secular cooling, concluded that the interior of the earth had shrunk more than mere cooling alone would account for, and suggested that this was due to the presence of superheated water in large quantities in the original nucleus, and that the blowing off of this water during volcanic eruptions might have contributed materially to the diminution of the volume of the magma.* In a subsequent work† Mr. Fisher has applied this hypothesis more particularly

* "Trans. Cambridge Phil. Soc.," vol. xii, p. 414.

† "Physics of the Earth's Crust," chap. xv, p. 185 *et seq.*

to the explanation of volcanic action. He supposes a solid crust of about 25 miles thick resting on a fluid substratum of highly heated rocky matter in a state of igneo-aqueous fusion, and shows that if a crack were produced by any cause in the under surface of the crust it would become filled with the water substance or vapour given off from the fluid magma at a high tension. Whenever the rent, commencing below, opens upwards, vapour at a high tension will escape, and after a certain time will be followed by the magma itself, which will overflow at the surface because the water-substance expanding, owing to the diminished pressure, will render the whole column of less weight than an equal column of the crust. On this view he considers that any disruption in the crust which is sufficient to permit the passage of steam at an enormous pressure, would originate a volcano; and "much of the lava poured out might consist of the materials of the crust itself, fused by the passage of the gases through it, and so vary in its composition at different vents, and even at the same vent at different times."

I need not dwell on the other objections I feel to these hypotheses because the special one before-named applies equally to this,—namely, that, if they were true, all rocks formed under such conditions should exhibit evidence of the presence or of the escape of vapour. All volcanic matter should be more or less scoriaceous, whereas there are many lavas which are little, and others not at all scoriaceous; while the great sheets of basaltic rocks which have welled out from fissures at former geological periods, are likewise neither scoriaceous, except very superficially if at all, nor are they accompanied as a rule by *débris* indicating explosions and projections due to the presence of vapour and gases. Why also should not all rocks of igneous origin, as well as volcanic rocks proper, be scoriaceous, if such were the conditions of the molten magma beneath the solid crust? The general want of hydration in volcanic rocks and their associated minerals is likewise incompatible with such conditions.

It has been contended by some writers that large subterranean cavities may exist at depths in the earth's crust, and that the vapour of water under high pressure may be stored up in such underground cavities. But the pressure of the strata is so great at depths, that, as in deep coal pits, where no permanent cavities can be formed, owing to the "creeping" and falling in of the strata, it would be impossible for such cavities to exist in Sedimentary Strata, while in Igneous rocks the initial plasticity of the rock and pressure would effect the same object. Even if such cavities did exist, they could only be maintained by the action of an elastic fluid, whose pressure would exceed that of the superincumbent strata. Geology affords no evidence of such underground reservoirs, or of any having existed in former times. No great explosions of pent up steam show themselves during the disturbances, shocks and rents accompanying earthquake

movements, and no persistent issue of steam gives countenance to the supposition that the water permeates the rocks to great depths or exists there in natural cavities.

Natural cavities at depths in the earth's crust I hold to be impossible. There may be cavities in the Igneous rocks near the surface, due either to contraction, to rapid cooling without pressure, or to the shell left by the escaping lava streams. But these cannot take place at great depths. They are connected with subaerial action.

With regard to such cavities as those so common and of such extent in limestone rocks, it must be remembered that these cavities are entirely due to the descent of the surface waters to a definite level, and to their escape by the most readily available outlet, either in adjacent valleys, or at or near the tide line on the adjacent coast. Below that level there can be no active circulation of water, and no possibility, therefore, of great cavities, due to the passage of water through underground channels, being formed. Changes of level may have carried some of these superficial cavities to certain depths beneath the surface, but that they should have been carried to the great depths we are referring to, or be of any sufficient size, is more than problematical. In limestone strata they occur near the surface, or at a short distance beneath the surface; wherever these rocks have been worked at a depth beneath the line of water saturation, such cavities are of very rare occurrence. Deep mines reveal occasionally a few fissures, and some comparatively small cavities, but these are in mineral veins, which show no relation with active volcanic phenomena.

§ 3. *Influence of Volcanic Eruptions on Spring and Well Waters.*

It is a singular circumstance that although the presence of water in volcanic eruptions has been so long recognised, and the disturbances caused to wells and springs have been so often noticed, no systematic series of observations has been made either on the surface or on the underground waters in connexion therewith. There are many allusions and incidental notices, but nothing in the form of special and exact details. Most writers on the subject speak of the disturbances to wells and springs as a common or obvious fact; but a series of extended and accurate observations is much needed.* In the absence of more exact data, we have to avail ourselves of general observations made by witnesses on the spot, amongst whom are many competent authorities.

The great eruption of Vesuvius of 1813–14, which commenced with a few trifling explosions and shocks in September, and by a small eruption of lava in October and November, followed by the great

* The observations should not be limited to the volcanic area, but should extend to the sedimentary strata around, and to some distance from the centre of eruption.

eruption of December, was witnessed by M. Menard de la Groye,* who remarks that "towards the end of May the well-waters of Torre del Greco and Torre dell'Annunziata failed, and that this was an ordinary precursory symptom of the eruptions." In June the waters continued further to lower, and "in the first fortnight in July they fell so low as to alarm the population," while "in October the wells of Resina, Torre del Greco, and other places failed in a surprising manner."

Professor Phillips briefly records† the following instances:—"July, 1804. Severe earthquake—diminution of springs." In May, 1812, the wells failed or were much lowered at Torre del Greco and Resina, as well as a thermal spring. In June, July, and August, heavy rains occurred; yet this did not restore the water in the wells, which still remained low, and even lower than before in September, and this scarcity was felt along the whole Vesuvian coast, and in the valley of the Sarno. Early in 1822 the wells lost their water. August, 1833, water failed in the wells.‡ The loss of water has sometimes been attributed to other causes, such as the state of the rainfall, &c., but Professor Phillips specially observes that this sinking of the wells cannot be explained by reference to the previous state of the weather;§ and, after a careful examination into all the phenomena connected with the eruptions of Vesuvius, he alludes again to "the sinking of water in the wells around Vesuvius—the total drying up of some, and the increased descent of the bucket in all," during times of volcanic disturbances, as an important fact.

M. Ch. St. Claire Deville‖ remarks: "It is well known that there is only one tolerably certain indication of an approaching eruption of Vesuvius, and that is the disappearance of the water in the wells of Resina and Torre del Greco."

According to Poulett-Scrope,¶ the threatening indications of an approaching crisis "are accompanied by the disturbance or total disappearance of springs, and such accidents as the cracking, splitting and heaving of the substructure of the mountain must naturally occasion."

Professor Guiscardi, of Naples, in answer to my inquiry, writes,** "As a rule, the water of wells in the neighbourhood of Vesuvius undergo changes in quantity, and even quite disappear before the commencement of eruptions. Only as well as I know in the eruption of 1861, the phenomena followed the eruption. I add a list of such diminishing and drying wells.

* "Journ. de Phys. et de Chim.," vol. lxxx, p. 390.
† "Vesuvius," p. 96 *et seq.*
‡ *Ibid.*, p. 140.
§ *Ibid.*, p. 141.
‖ "Bull. Soc. Géol. de France," 2nd Ser., vol. xiv, p. 254 (1856).
¶ "Volcanos," 2nd Edit., 1862, p. 21.
** Letter to the author dated 1st Sept., 1881.

"1843. Decrease of water in the wells of Resina; it was preceded by emission of lava.

"1846. Some wells of Resina dried, and emission of lava followed.

"1846. Six adventive cones in the crater; water decreases at Resina in wells.

"1847. Decrease of water in the wells of Resina; great lava flowing.

"1848. Water decreases in the wells of Resina and Torre del Greco. Earthquakes in the neighbourhood of Vesuvius. Lava flowing.

"1849. The same decrease of water—strong explosions, bellowing, and lavas.

"1850. January 23rd, at Resina and Torre del Greco decrease of water in wells. Strong explosions. February 5, lava poured out with bellowing.

"Before the eruption of 1794, there was at Torre del Greco a small torrent, capable, it is said, of moving four mills. After the eruption the torrent got very poor, so that the water scarcely supplied a fountain. After the eruption of 1861, there was an increase of water in this fountain, and in some small springs near the shore, and one was noticed in the sea itself, which lasted nearly a month.

"There is a well on a farm of some relatives of mine, at St. Georgio di Cremano, in right line nearly 4 miles from Vesuvius, 150 feet deep, and plentifully fed by a spring. After the eruption of 1861, the water began to decrease, and a year after it was quite dry. This was followed by so abundant an emission of carbonic acid, that the well had to be stopped up."

I must observe, however, that the high authority of Professor Palmieri is against this view.* He states that previous to the eruption of Vesuvius in 1871–72, the water in the wells was neither deficient nor scarce, but was very acid afterwards. He elsewhere mentions that he considers these supposed premonitory signs either only to happen occasionally, or to be mere coincidences, such as the coincidence of a dry or rainy season. But the weight of evidence is certainly against this opinion, and, as I shall presently explain, there may be tracts that have an independent water-level which escape the surrounding disturbance, and it is not impossible that this very circumstance has led to the selection of such areas for the sites of towns and villages on the slopes of the mountain.

The more local springs which supply the shallow surface wells may remain undisturbed, while at other points the deeper-seated springs having a wider range may be tapped and drained. Again, the water in

* "The Eruption of Vesuvius of 1872," translated by R. Mallet, F.R.S., p. 135.

the superficial volcanic strata will usually flow towards the circumference of the mountain, in consequence of the beds by which they are held up dipping from the central crater; while the springs in the sedimentary strata lie in the continuous planes traversed by the volcanic duct, and towards which they may often dip. Some irregularity in the phenomena is therefore to be expected, and while in most places the wells suffer, it is quite intelligible that in others they may be but little affected.

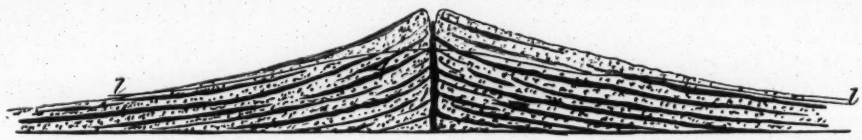


Fig. 1.—Diagram section of a volcano. The dark bands represent lava-streams; the dotted spaces, ashes and scoriæ; and the line *l*, the water level in the mountain.

As bearing upon the subject of the disturbances to which water-bearing strata are liable in volcanic districts, we have the evidence of M. Mauget, a well-engineer of great experience, respecting the sudden changes of water level in the Neapolitan area, during his residence there. There were eruptions of Vesuvius in 1865 and 1867, but none in 1866. Nor were there any important earthquakes, but a number of minor ones are recorded.

M. Mauget says that in May, 1866, the wells and springs around Naples began to be affected, and continued to diminish until June, but considers that this might be due to ordinary causes, such as lesser rainfall. On the 29th June a sudden change took place. The waters of the aqueduct, which brought in the water from a distance of 12 miles, and of the canal of Lagno di Mofita, as well as of various rivers, became troubled and reduced in a surprising manner. The next day the waters became bright, but were found to be reduced to the extent of one-fifth of their volume. The great springs of the Sannio district were reduced by one-third; and the town of Sorrento was deprived of all potable water. This water is brought in by an aqueduct from the neighbouring hills, which consist of Eocene or of Cretaceous strata. The whole district, from the foot of the Apennines to the Neapolitan coast, was affected over an area of 66 miles square.

At the same time various artesian wells in the valley of Sebito became sanded up and greatly reduced in their flow, and the two deep artesian wells of Naples threw up above 200 cubic mètres of trachytic and pumiceous sands and lapilli.*

It is evident that this diminution in the surface waters could only

* "Sur les variations subites dans le régime de divers cours d'eau dans l'Italie Méridionale," "Comptes rendus," vol. lxiv, p. 189 (1867).

have been caused by their absorption under ground, either to restore some water level reduced by a former eruption, or to fill fissures in course of formation preceding the eruptions of 1867 and 1868.

It seems to me, therefore, to use the words of Professor Phillips, that the observations respecting the effects produced on wells and springs by volcanic eruptions and earthquakes, "have been too often and too carefully made to allow of a serious doubt on the subject;" he asks, "What is the cause of it? and why is it an indication of coming disaster?"

§ 4. *The Hydro-geological and Statical Condition of the Underground Waters in and under a Volcanic Mountain.*

The cause is, I believe, not far to seek, when the hydro-geological conditions of the strata composed above of volcanic matter, and below of sedimentary strata, are considered.

So well known is the absorbent power of a volcanic surface, that the mention of the fact hardly seems necessary, except in corroboration of subsequent statements and for the purpose of independent testimony. On ordinary strata it is roughly estimated that about one-third of the rainfall passes under ground, but on volcanic surfaces the whole rainfall soon disappears, a small proportion only being lost by evaporation. Amongst innumerable notices of this fact, it will suffice to mention those of two experienced authorities. Lyell remarks on the dry and arid surfaces of Etna, and on the rapid absorption of the rainfall, and observes that "the volume of rain-water and melted snow commonly absorbed by a lofty mountain like Etna, is enormous;* again, Piazzi Smyth, in describing his ascent of Teneriffe, says, "that though so much rain had fallen lately, not a trickling stream, not even a drop of standing water, was anywhere to be seen; the pumice-stone ashes had swallowed all up." †

Volcanic mountains being composed of streams of lava of very variable width and length, irregularly alternating with more widely spread layers of scoriæ and ashes, the whole mass would be permeable were it not that the decomposition of some and the consolidation of other beds, by atmospheric and aqueous agencies, have formed here and there impermeable beds, which hold up the rain-waters, and furnish local supplies to wells and springs. But where such impermeable beds do not intervene, the rain-water penetrates to greater depths, and is there stored until the line of water-level reaches to such a height that the hydrostatic pressure forces it outwards, and causes it to escape at the points *l* as springs either temporary or perennial (fig. 1).

This storage may take place either in the lava or in the beds of scoriæ and ashes. Solid lava is impermeable, but water penetrates

* "Phil. Trans.," vol. 148 (1858), p. 763.

† "Teneriffe," p. 349.

through, and is held in, the numerous fissures and cavities by which it is traversed. These fissures are due to contraction on cooling, and to the fractured state of the lava produced by the splitting caused by subsequent disturbances, whilst larger cavities are produced by other causes. Of these, the two most important are—1st, the escape of vapours while the lava is consolidating. Sometimes the hardened outer crust of the lava is raised in great blisters, which, on the escape of the vapour, are sufficiently solid to retain their position, and remain like so many empty beehives on the surface of the lava streams. The Grotta delle Palombe, on Etna, which, according to Waltenhausen, has a length or depth of about 500 feet, and a height in places of from 70 to 80 feet, and the great ice cave near the top of the Peak of Teneriffe, described by Piazzi Smyth,* and so large as to contain a lake of water of considerable size, are attributed by them to the escape of elastic vapours.

2nd, the escape of lava from a lava stream after the exterior of it has become solid, when an empty shell in the form of a cave or tunnel is left. These tunnels or caverns are of common occurrence, and often of large size. Scrope observes† that "among the lavas of Etna, Bourbon, Iceland, St. Michael, Teneriffe, and many others, caverns of very large dimensions are thus formed beneath the surface of a lava stream, and often imitate in their extent and windings the well-known caves worn by water in limestone rocks." Phillips and others notice the occurrence of similar tunnels in the lavas of Vesuvius, but they are all small.

In the great volcanic mountains of South and Central America, Humboldt long ago inferred that large cavities filled with water must exist in consequence of the ejection of water, with small fishes and tufaceous mud, from fissures caused by the earthquake shocks which precede the eruptions of the volcanoes in the Andes.‡

A French geologist, M. Virlet d'Aoust, has, moreover, given particulars of two great tunnel-caverns of Central America,§ which will serve to indicate the magnitude of some of these subterranean reservoirs. The first is that known as the *Cueva de Chiuacamoté*, near Pérota, which he was assured extended several leagues in length (!) He found it to be a cavern of great size, and divided into compartments by falls of the roof. The floor is covered with a sandy gravel, and the side walls, here as in the cave of Custodio, exhibit grooved lines covered with slight calcareous incrustations indicative of old water-levels. The other is the *Breña de Custodio*, in the State of San-Luis Potosi, of which he says that it forms a perfect semispherical tunnel

* "Teneriffe," p. 352.
† "Volcanos," 2nd Edit., p. 79.
‡ "Cosmos," Sabine's Translation, vol. i, p. 230.
§ "Bull. Soc. Géol. de France," 2nd Ser., vol. xxiii, p. 34.

of the size of our largest railway tunnels, at its end dipping towards the centre of the mountain.

Cavities originating in these ways must have been formed at all times and in many lava streams, and although a certain number of them, especially those due to the upward escape of elastic vapours, may have been filled up by succeeding lava streams, this would not be the case with tunnel-caverns opening downwards. Nor would these streams always fill up even open fissures, as they push before them a mass of solidified *débris*, which forms a pavement protecting the underlying mass.

The lava throughout a volcanic mountain may therefore contain a greater or lesser number of caverns, which serve, whenever they happen to lie below the normal line of water-level, as so many reservoirs. The mass of the lava is further riddled with fissures of all dimensions, which act as water-channels and channels of intercommunication.

Again, the beds of scoriæ, ashes, and tufaceous deposits serving to build up volcanic mountains, and which overlap the lava streams, and extend to considerable depths, are often water-bearing. Some contain powerful springs, like stratum No. 4, which was met with in the Palace well at a depth of 368 feet beneath the surface at Naples (p. 141). The shallow surface wells of the district are commonly in beds of this character.

Even the more impermeable tufaceous beds contain cavities which when under the line of water-level, must serve as reservoirs. These cavities, which attain a size of 2 feet or more in height, and are lengthened out in a vertical direction, like the flues of chimneys, have been formed by the disengagement of elastic vapours during the consolidation of the beds, that consist, in the Naples district, of volcanic tuff with trachytic and other rock pebbles.* These beds have a wider extension than the lava masses, which further decrease in importance as they trend from the central area of eruption.†

The dykes running in vertical lines through volcanic mountains form another structural feature having an important bearing upon the question under consideration, for they traverse radially the beds of ashes, scoriæ, tufa, and lava wrapping round the central duct, with which they serve to place them in communication. Besides these great radial dykes, which are often extremely numerous, there is a network of small fissures or dykes branching off from them in all directions.

During the eruption of Etna in 1865, a rent was formed at the

* Dufrenoy, "Ann. des Mines," 3rd Ser., vol. xi, pp. 113, 120 (1837).

† Some volcanic mountains are, however, composed almost entirely of ash and scoriæ beds, and others of lava streams; the line of water-level will be modified in accordance with these conditions.

crater of Frumento, which extended in a direction away from the central cone for a distance of 1½ mile. Scrope says that in nearly every lateral eruption of Etna, the production of such a fissure has been observed. Similar instances are not wanting in Vesuvius. In 1738 a fissure crossed the whole island of Lancerote; while in the great eruption of Hecla of 1783 the fissure which was then formed was supposed to extend not less than 100 miles in length.*

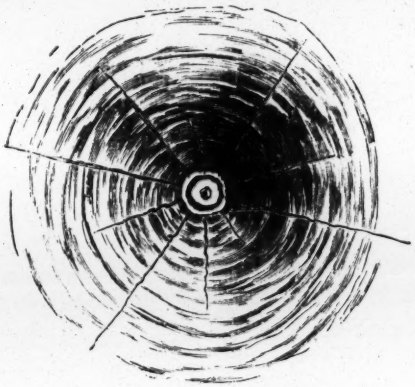


Fig. 2.—Diagram plan of a volcano, showing the radial lines of fissures or dykes.

Scrope further remarks that "the rents thus produced in the frame of a volcanic mountain are sometimes of such a size as to cleave its whole mass in two. This occurred in the volcano of Inachian, one of the Moluccas, in 1646. The crater of the Soufrière of Montserrat, and the volcanic cone of Guadaloupe both appear to have been thus split through. So also the Montagne Pélée of Martinique." Piazzi Smyth states that the cinder beds surrounding the summit of Teneriffe are traversed by dykes proceeding in radial lines from the Peak.† Phillips describes some of the dykes and fissures of Vesuvius, and gives a section showing the relative position of the ejected *débris* and lava beds of this mountain, and of the dykes radiating from the central core.‡

These dykes, like other masses of lava, have generally become fissured in cooling, and the interstices thus formed serve as water channels, not only for the rain which falls upon them, or to small springs high up the mountain as in Teneriffe,§ but, what is more important, they may serve as channels of drainage to the water-bearing scoriaceous and lava beds which they intersect. For it will be understood from the preceding account that these latter beds may often

* "Volcanos," 2nd Edit., 1862, pp. 161—163.
† "Teneriffe," p. 80.
‡ "Vesuvius," pp. 132 and 191, and Pl. VI.
§ "Teneriffe," p. 86.

form independent and isolated water-reservoirs; but, traversed as they are by dykes communicating with the central mass, these dykes serve as so many conduits to carry the water from the separate water-reservoirs and drain them into the central duct (fig. 2) whenever the normal hydrogeological conditions are disturbed. At such times the dykes therefore contribute greatly to the discharge of water into the interior of the volcano.

Very little is known of the substrata of a volcanic mountain. We know that Vesuvius, Etna, and Hecla stand on Tertiary strata—that some volcanoes in America stand on Cretaceous or Jurassic strata, and others probably on crystalline rocks, but of the stratigraphical details underground we have very scanty information. The only instances that I am acquainted with are the sections obtained in boring for the two artesian wells constructed in 1865–66 at Naples by MM. Degousée and Laurent, of Paris. These supply very important data not only respecting the volcanic beds, but also respecting the sedimentary strata beneath. One well is situated in the Piazza Villa-Reale, and was carried to a depth of 1106 feet, and the other, in the gardens of the Royal Palace at Naples, 72 feet above the sea-level, was carried to the depth of 1524 feet. The details given of this latter by M. Laurent are as follows :—*

Section of Artesian Well in the Palace Gardens, Naples.

		Thickness. mètres.	Depth. mètres.
Volcanic ejecta-menta.	1. Soil and made ground	16·50	16·50
	2. Yellow volcanic tuff	52·50	69·00
	3. Green „ „	33·00	102·00
	4. Volcanic ashes, in places argillaceous, and containing numerous pebbles of trachyte ..... *1st spring*	103·40	205·40
	5. Greenish volcanic tuff	7·00	212·40
	6. Grey clay.....	8·10	220·50
	7. Grey marly tuff with trachyte	4·00	224·50
Sub-Appenine strata.	8. Sandy marl with veins of lignite	25·00	249·50
	9. Grey marly and bituminous sands, with mica. *2nd spring*	27·00	276·50
	10. Hard sandstone	1·80	278·40
	11. Compact shelly marl	44·80	323·20
	12. Alternating micaceous sands, soft sandstones and carbonaceous marl....*3rd spring*	48·70	371·90

* "Guide du Sondeur," 2nd Edit. 1861; vol. i, p. 137; ii, p. 496; and Pl. L.

		Thickness. mètres.	Depth. mètres.
Sub-Appenine Strata, *continued.*	13. Micaceous marl and siliceous limestone	7·81	379·70
	14. Compact micaceous marl ..	53·19	432·90
	15. Compact marl with layers of limestone	25·72	458·62
	16. Argillaceous limestone	2·00	460·62
Eocene.	17. Macigno (a hard calcareous sandstone) ..... *4th spring*	4·08	464·70

No ascending spring was met with until the volcanic ashes stratum No. 4 was reached at a depth of 368 feet. Another spring was found at a depth of 830 feet in the Tertiary marly sands No. 9; another in the micaceous sands No. 12 at 1106 feet, but no spring of the desired volume was met with until a sandy bed under the Macigno was reached at a depth of 1524 feet. The discharge of water from this bed amounted to nearly 2 cubic mètres per minute, and rose about 30 feet above the surface, so that the water could be used as a natural fountain in the Palace gardens. In the Piazza the artesian waters formed another natural fountain rising 8 feet above the surface.

These wells, therefore, show the existence of one important spring in a stratum of volcanic *débris*, and of three springs in the sedimentary strata beneath. But this only gives the more powerful ascending springs; bodies of water of lesser volume, or which do not rise to the surface, escape notice in works of this description.

The overflow of the water from the bed of volcanic ashes proves that the bed comes to the surface at a level higher than that of the ground where the well is situated, and as the water has sufficient ascensional force to rise several feet above the ground, it must necessarily, in order to overcome the resistance or friction of the bed through which it passes, stand at its outcrop in the central volcanic area considerably higher than at the point of overflow. There must, therefore, be a continuous sheet of underground water, the level of which rises towards the centre whence the water-bearing bed proceeds. In this way all the permeable beds of a volcanic mountain will be charged with water, the level of the water rising with the distance from the point of escape and with the height of the mountain. Everywhere beneath the level of saturation the surface-waters will eventually fill all fissures and interstices, and lodge in them permanently, unless disturbed or drawn off by artificial or natural means (see fig. 1).

This level depends, when there are alternating permeable and impermeable beds, on the height of outcrop of the former, and, when the whole mass is permeable, on the texture of the rock, or the friction to which the water is subjected. In the Chalk hills of the south of England which are composed of a comparatively homogeneous

rock, but variably fissured, the rise of the line of water-level varies from 13 to 150 feet in the mile, and in some strata it is even more.

It will therefore be apparent that in the case of the irregular and complex beds forming a volcanic mountain, the height of the water-level is subject to too many conditions to be determined accurately, except by experiment, and for this few opportunities present themselves. There is, however, an available natural datum line, namely, that furnished by the escape of springs, at certain high levels on the mountain slopes.

As springs issuing from a body of homogeneous strata or strata which intercommunicate, depend for their permanence upon the water stored up in the interior of the mountain, at a level above that of the point of escape, it follows that if there is a point of permanent escape, we may conclude that in the ground behind, all the strata below that level are under the line of permanent saturation, and therefore charged with water; and further, as just said, this line of permanent saturation must stand the higher the further it goes into the body of mountain.

Now, on Etna, Wattershausen* describes a spring in the valley of St. Giacomo, near Zafarana, which he says is the only point at so high a level at which a tolerably strong spring constantly issues. It at once forms a small waterfall, and runs some distance until lost in some volcanic sands lower down. The strata, where they issue, are composed of alternating layers of tuff and compact lava. Its escape is due to the circumstance that there is here a ravine which cuts through the bed, and either touching on the line of water-level or intersecting the junction between an impermeable and a permeable bed, and thus taps the subterranean waters. Wattershausen does not give the height of the ground, but from a section of Abich's, which passes near the spot, I infer it to be about 2000 feet above sea-level.† On the other side of Etna the river Simeto, or one of its tributaries, rises near Bronte, at a height of about 2200 feet above sea-level. This likewise indicates the existence of a perennial spring. I find also that at other points around the mountain at and about this level, streams commence which point to a like line of water-level.

These figures are only roughly approximate, but they constitute our only available data, and in taking a mean level of 2000 feet, I believe I am rather below than above the mark. If, therefore, we take Wattershausen's section of Etna, supplemented by Abich's, which follows nearly the same line, and gives, moreover, the height of the several points, the following diagram would represent generally the *massif* of the mountain and the position of the line of saturation.

From the points of permanent issue which we have noted at Zafa-

* "Atlas de l'Etna," Part I and Pl. V.

† Vesuvius and Etna, 1837. Sections of Etna.

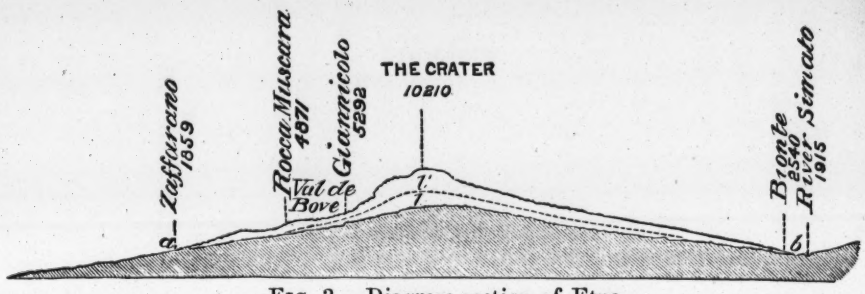


FIG. 3.—Diagram section of Etna.

rana and near Bronte, the line of water-level will rise in proportion as the ground rises between *a* and *b*; and in the same way in the great central dome which rises rapidly above the lower slopes, the water-line will rise more suddenly. It is therefore possible that in the centre of the mountain the line of permanent saturation, *l*, may occasionally stand higher, *l'*, than the top of the Val del Bove (5292 French ft., Abich). It may lie too deep for springs to issue, but the remarkable flood in that great depression described by Lyell,* and connected with the eruption of the year 1755, seems explicable on the supposition of a high central water-level more readily than on the assumption of the sudden melting of a mass of ice in the interior of the mountain. For although ice may be formed and retained under a covering of lava in the manner described by Lyell near the summit of Etna, the cold would not penetrate to a sufficient depth to allow of the accumulation of a mass of ice of the dimensions required for so great a flood. Any body of ice must be superficial, as the increase of heat with the depth from the surface would be a bar to its existence in the interior of the mountain, independently of the heat diffused at all times from the central duct. Nor would the sudden melting of the snow, which never lies very deep, and would frequently recur, explain the sudden great and exceptional outburst of flood-waters described by contemporary writers.†

On the other hand, heavy rains or the prolonged repose of the volcano, may have resulted in the exceptional rise to *l'* of the level of the underground water-line, *l*, so that if one of these radial fissures, so frequently formed during the eruptions of Etna, suddenly opened in a direction to traverse the water-logged strata, the effect would be to tap and drain at once the whole of this subterranean reservoir to the level of the point of escape—in the Val de Bove—a point still about 5000 feet below the summit of the mountain; while the water, coming as it would from the centre of the mountain, would also account for its reported heat.

* "Phil. Trans.," vol. 148 (1858), p. 68.

† Canon Recupero, who reported on the catastrophe, came to the conclusion that the water was vomited forth by the crater itself, and was driven out from some reservoir in the interior of Etna (Lyell, *op. cit.*).

This intersection of the line of water-level in Etna was probably due to the peculiar shape of the mountain, and the rapidity of the slopes above the Val del Bove. In volcanic mountains generally, this level is too depressed to be within reach of surface irregularities and fissures.

With respect to the condition of the sedimentary strata under a volcanic mountain, very few observations have been recorded, M. Constant Prevost, after visiting most of the volcanic districts in Europe, concluded that they were not in general much disturbed—that the volcanoes were on lines of fissures but not on lines showing much lateral compression, or on anticlinals. If we may take Vesuvius as a type, we should conclude that the sedimentary strata pass under the mountain and crop out in the adjacent sea-bed with little interference from faults. For the tertiary strata under Naples, come to the surface on the hill ranges further inland, and dip continuously seaward; and the fact of the overflow of water from the water-bearing beds—water of course passing down from the outcrop on the surface—to the height it actually attains, shows that the continuity of the beds cannot be materially interrupted.

The well-sections at Naples determine the existence of at least four water-bearing strata, of which the deeper one (1524 feet) discharged about 600,000 gallons daily, rising 102 feet above the sea-level. So great a pressure would show that this stratum had its outcrop inland at a considerable elevation above Naples, and the volume of the spring would prove that the head of underground water above the line of sea-level was large. Consequently, as the plane of this water-bearing bed must be traversed under Vesuvius by the volcanic duct, the sides of that duct or fissure will at that point be subject to the amount of hydrostatic pressure indicated by these conditions, or to a pressure, apart from friction, equal to that of about 53 atmospheres.

With respect to the ordinary mode of escape of the underground waters, that portion held in the more superficial volcanic beds will escape as springs on the slopes or at the foot of the mountain; but with respect to the underlying sedimentary strata—when they are adjacent to the sea and crop out in the bed of the sea—the surplus waters (or those annually added to the underground stores by the rainfall) will escape, 1st, in springs flowing on the surface; 2nd, by springs issuing in the sea-bed—and the size of these springs will depend on the hydrostatic pressure, and on the resistance and friction of the conducting strata. When the water-passages are contracted, as in sand and sandstone, the submarine springs will be small and slow: but when large and more open, as in limestones, the discharge in the sea-bed will, as on the coast of Spezzia and elsewhere on the Mediterranean, form large and powerful springs of fresh water rising through the waters of the sea.

Such are the hydro-geological conditions of a volcanic mountain in a state of rest. The effects when that equilibrium is destroyed will now be discussed.

§ 5. *Condition of the Underground Waters during an Eruption.*

So long as the volcano remains in a state of rest, so long will the hydro-geological conditions described in § 4 continue unchanged. The level, *l*, of the underground waters may rise; but, as in the meantime cooling has, by causing solidification, isolated the molten lava, no result is produced except that arising from the trickling of the surface-waters on the yet hot surfaces, giving rise to the small columns of steam common during the periods of rest. After a prolonged period of repose, even these minor effects cease. It may happen that if the crater is very deep and there has been a long interval since the last explosion, that the water-level may mount into the crater and give rise to a lake. In most cases, however, such lakes are due to the relatively low position of the crater with reference to a part of the adjacent ground, or to the decomposition of the lava, whereby it has become retentive of the rainfall. Such bodies of water are important in the event of any fresh eruption.

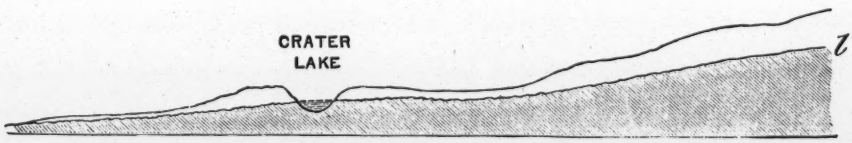


Fig. 4.—Crater Lake dependent on the general water-level (*l*).

Crater lakes are not so common or of so large a size in Europe as in Central America. The lake of Atitlas, 1558 mètres above the level of the Pacific, is 20 × 15 kilomètres large, and of a depth yet unascertained. The crater lake of Masaya in Nicaragua is 8 kilomètres across, and 150 mètres deep in the centre. In 1852 this lake suddenly appeared as though boiling, and a violent explosion shortly followed. The volcano of San Salvador is 2300 mètres high, and at the bottom of its crater, which is 700 to 800 mètres in diameter, and 400 to 500 mètres in depth, is a very deep lake. Another large crater lake, 12 kilomètres west of San Salvador, is level with the ground and 200 mètres deep.*

The crater lakes, however, are an exceptional feature. The great body of the rainfall becomes stored within the mountain itself, and makes itself visible only by springs on the lower slopes of the

* "Voyage Géologique dans la République de Guatemala et de Salvador," par MM. Dollfus et de Mont-Serrat, 1868, pp. 103, 106, 318-20, 374-5.

mountain. Its distribution must be irregular from the circumstance that volcanoes consist of alternating beds of permeable scoriaceous and of solid or decomposed impermeable materials, and as each of these is of limited extent, there may be a number of independent water-levels. It consequently follows that one may be affected by the disturbances accompanying an eruption, while others adjacent do not suffer, or suffer but little. It is only when these several levels are traversed by the same set of rents that intercommunication is established. The following section shows how numerous are the beds on the flanks of a volcano, and how they are traversed by dykes.

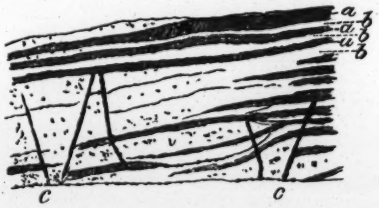


FIG. 5.—Section on the slopes of the old Volcano of Santorin (Fouqué).
a. Lava flows; *b*. Permeable scoriæ and ashes; *c*. Dykes.

The hydro-geological conditions of a volcanic mountain during the period of repose are of the same character as those ordinarily obtaining in Sedimentary strata, but whenever an eruption takes place these conditions are disturbed, and a special class of phenomena ensue. (See Plate 1.)

After a period of rest, the chilled superficial plug of lava in the volcanic duct will be of a thickness proportionate to the duration of that period, and when the upward pressure of the molten lava beneath begins to exert itself, the resistance will be in a ratio to the thickness of the plug. If thin, it will soon yield; small cracks and fissures will be gradually formed, into which the water lodged in the beds around, or may be in the crater, will find its way by slow degrees, causing an increase in the discharge of vapour, and giving rise to detonations, small at first, but increasing gradually as the heated lava breaks through the crust. Should, on the other hand, the plug be thick, it will not yield so readily to the pressure of the ascending column of lava; but when the tension has reached a certain point it will rupture more or less suddenly, forming deep fissures through which the water will be precipitated into the molten lava beneath, in quantity sufficient to produce those powerful and paroxysmal explosions with which the eruptions usually commence after lengthened periods of rest.

As a consequence of these detonations and explosions the fabric of

the mountain is shaken—the underground waters suffer disturbance, old water-channels are sanded up, new channels are formed—and the waters are dislodged and driven to seek fresh beds and fresh lodgments. These effects are necessarily most strongly felt in the rocks around the centre of eruption, and it is there that they undergo the maximum of displacement and fissuring; and as the water lodged in that part of the mountain flows into the volcanic duct, it flashes into steam, and is driven out in continuous explosions. Thus, the water stores immediately surrounding the central duct and crater, become gradually exhausted, and their level is more or less lowered. As this loss proceeds, the water lodged in more distant parts of the volcanic mountain flows in to supply the void, and the explosions will be violent and prolonged, according to the available volume of water present in the mass of the mountain. As these central water stores are driven off and become exhausted, and the level of the underground water is gradually lowered, there must necessarily be an influx from the circumference of the mountain to replace the loss caused by the central explosions, and this will continue so long as the central body of water is kept by the eruption lower than in the strata and fissures at a distance. These different hydraulic conditions are represented by the diagrams—sections 1, 2, 3, Plate I.

In these diagrams v represents the mass of volcanic materials; S the sedimentary strata; p the permeable strata; m the impermeable strata; a, b, c the normal level of the underground waters in the volcanic mountain; and l their level in the sedimentary strata. The arrows show the direction in which the waters flow under the normal and also under the altered conditions caused by the eruption. In section No. 1 the points a and c are fixed levels, while b fluctuates according to the amount of rainfall and to the length of the intervals between the eruptions. Its height above a and c depends upon the distance from those points and on the resistance of the materials through which the water percolates. This of course is apart from any exceptional interfering causes. The flow of water in v, section 1, will, under normal conditions of repose, be from the centre towards the circumference, and it will escape at the lowest levels; and if there are no lower levels inland, the whole direction will be seaward. In this section, however, the depression at c allows of an escape of some of the water on the inland side of the mountain.

As the level of b is lowered by the discharge of water into the duct of the crater, the water first ceases to escape at a and c, and then the adjacent body of water below that level sets in with an inward flow towards the central duct, as shown in section 2; for it is a well-known fact that in masses of partially resisting strata, such as the Chalk or the New Red Sandstone, excessive pumping (or the removal of water faster than it can be replaced from the surrounding strata) causes

a depression in the line of water-level in the form of an inverted cone, large in proportion to the amount of friction, and this continues until, on the cessation of pumping, the water gradually recovers its normal level by influx from the surrounding beds. The discharge of water from the crater is an analogous operation, and must be attended with the same consequences.*

This flow of water towards the volcanic centre is greatly facilitated by the dykes radiating from that centre, and which, intersecting at right angles the several water-bearing masses, serve as conduits to carry the water into the main duct of the volcano. Nevertheless, there may be some areas so isolated as to escape. If, however, a large fissure were formed through the head of water *ab* or *cb*, and happened to open out on the slope of the mountain at a level lower than the water-level in the interior of the mountain, then the escape of water would be outwards, and its volume would be in proportion to the mass of the mountain above that level, or to the height of the water-level at *b*. Such a fissure, probably during an exceptionally high water-level, might account, as before mentioned, for the flood on the slopes of Etna in 1665, and it is possible that the same cause may have produced some of the great water and mud discharges recorded of other volcanoes.

The progress of the eruption by which the level of *b* is gradually lowered finally determines the whole of the available drainage of *v* into the central cavity or duct: and if any portion of *v* be below the sea-level, and that portion contain any permeable beds, or if it be traversed by any of those radial dykes, which, at the high tide of *b*, carried its waters into the central duct, then whenever the level of *b* descends below the point of the escape of the fresh inland waters into the sea, the same permeable strata or the same dykes will serve to carry the salt water from the sea to the central area.† So also, should any fissures, under these circumstances, open at the sea-level after the higher inland head of water is drained, then the water from the sea will flow into that fissure and be carried inwards into the mountain. Professor Moseley mentions‡ a submarine eruption, that in 1877 took place off the Hawaiian coast, in a depth of from 150 to 400 feet of water, 50 miles from Mauna Loa, during which a fissure, in all

* The vapour of water constitutes by far the largest part of the elastic fluids given off during eruptions, probably $\frac{950}{1000}$ or even $\frac{999}{1000}$ of the whole. M. Fouqué estimated that the quantity of vapour projected from Etna in the eruption of 1865 amounted to the large quantity of 22,000 cubic mètres, or about five million gallons daily.

† Just as wells adjacent to the coast and deeper than the sea-level are subject to an influx of sea water if the pumping is carried too far, or the level of the springs too much lowered.

‡ " Notes by a Naturalist on the ' Challenger,' " p. 503.

probability connected with this eruption, opened on the coast. This fissure was traced inland from the shore for nearly 3 miles, varying in width from a few inches to 3 feet. "In some places the water was seen pouring down the opening into the abyss below." This is not strictly an analogous case, but I mention it to show how the activity of the eruption may be promoted by the influx of surface waters.

Similarly, should the water-level in and under the mountain fall below both the sea-level and also below the general level of the water, *l*, in the permeable strata *p*, then not only will the springs at the surface of *p* fail and the wells run short or dry, but the outward and seaward current will also be reversed, and the water will flow in from the sea to the seat of the volcanic disturbance, through the same channels as those by which the inland waters before escaped. This later condition of the eruption is represented in section 3, where the level of *l* in the strata *p* falls below the sea-level. With the fall of the water-level, *l*, the available supply of water becomes gradually exhausted or the channels of communication impeded, and this continues until, with the cessation of the extravasation of the lava, the eruption comes to an end.

To return to the first stage of the eruption. The lava, as it rends and crashes through the plug, comes into contact with the water lodged in the cavities and porous strata about and above the plug; explosions and detonations follow, violent in proportion to the supply of water. More or less of the lava is hurled into the air and scattered as scoriæ and ashes, and the explosions continue so long as water finds its way to the escaping lava, but as the supply becomes gradually exhausted the detonations diminish in power and number, until they finally cease from want of supply. But the extravasation of the lava may, and often does continue, long after this exhaustion of water supply, showing, as I believe, the independence of the two causes; for otherwise the flow of lava would be accompanied to the last with explosions and detonations due to the escape of the extruding agent.

This flow of water from the surrounding beds into the volcanic duct, its sudden flashing into steam and the violence of the explosions during the first period of the eruption, are easy to conceive; but greater difficulties attend the following stages when the column of lava has ascended higher and fills the duct, and the level of the underground water has become lowered. The only way water can then gain access is through the walls of the duct into the fluid lava as it ascends.

This involves some little-understood problems. Of the actual underground conditions we must ever be ignorant, and experiment at present guides us but a short way. Any inquiry must therefore for

the present be more or less conjectural. There can be little doubt that (as before explained) when the volcano is in a state of rest, the beds of volcanic materials surrounding the upper part of the volcanic duct are charged with water to a certain height, and also that, when the volcano stands on sedimentary strata, the duct lower down traverses a certain number of water-logged strata, where the hydrostatic pressure is considerable. For the water to rise to the height which it does at the Naples wells requires a pressure of not less than 50 atmospheres; but the static pressure of the column of lava in the crater of Vesuvius at the depth at which the water-bearing stratum there lies (1520 feet), during a central eruption of the lava, is, of course, far in excess of this. The introduction of the water into the lava must therefore depend on other conditions than hydrostatic pressure, such, for instance, as capillarity, or the elastic force of vapour. Although this increases so rapidly, yet as the law of increase at temperatures exceeding 436° F. has not been experimentally determined, and we have to deal with temperatures approaching to or equalling the melting point of lava, which is not less than 2300° F., we can only infer what may possibly be the consequences of the passage of a volcanic duct through water-bearing strata; and our remarks on this point must to a certain extent be taken as merely suggestive.

Under great pressure and friction water may continue to circulate underground until its critical point is reached, or until a point is reached when the elastic force of vapour, or of its disassociated gases, exceeds the hydrostatic pressure. In the former case, taking the critical point of water at 773° F., the depth would be about 35,000 feet, but of the limits of the latter we are ignorant.

If at a depth say of 5000 feet, or at any other depth, a water-bearing stratum should underlie a volcano, the temperature of the water in that stratum will, independently of its temperature of depth, rise rapidly as it approaches the volcanic duct, and pass progressively through an ascending scale until a temperature of 700° to 800° F., or higher, is reached. We will assume that at some point the force of the elastic vapour counterbalances the hydrostatic pressure, and stays the further approach of the water. In this case, and supposing the volcano to be at rest, the only underground effect would be that the water to a certain distance, *n*, fig 6, from the duct, would under that high temperature be at its critical point, or in some state of maximum tension and pressure. This being effected, there will be no further change so long as a state of equilibrium is maintained, and the pressure of the lava at rest in the duct D remains equal to the elastic force of the superheated water or vapour in *n*. If, however, that state of rest is disturbed, and the lava in the duct begins to move upwards, as represented in fig. 7, then, whatever the

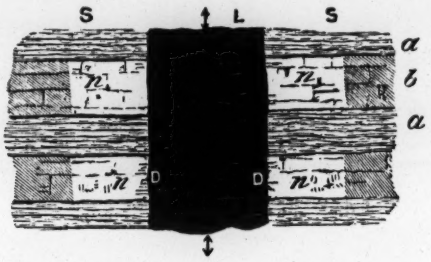


FIG. 6.—Lava column in a state of rest.

S. Sedimentary strata; *a.* impermeable; *b.* permeable. L. Lava in duct.

pressure may have been, the change from the static to the kinetic immediately destroys the balance; the lateral pressure of the lava, L, in the duct is no longer equal to that of the superheated vapour in *n*, which is therefore driven into the yielding mass of molten lava and so ascends to the surface.

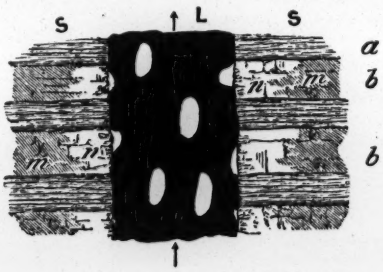


FIG. 7.—Lava column during flow.

As the escape of this vapour of maximum tension relieves the pressure on the water in the shaded part *m*, a portion of this water is at once driven in to replace it; and so long as the pressure of the column of lava in the duct is less than that of the vapour or water in the state in which it exists at *n*, so long will successive increments of the vapour be driven into the lava and cause continuous explosions.

The only limit to the depth at which the introduction of water into the duct can be effected will be that at which the general underground percolation of water is stayed in the manner before explained—that is, by the increase of the temperature due to central heat—a depth far in excess of that where we are supposing the introduction of the surface waters to take place.

With regard to the lesser depths and lesser general temperature of the water in the strata surrounding the higher parts of the volcanic duct—capillarity will there continue to force the water forward so long

as the underground water stands at a sufficiently high level. Mallet was of opinion* that capillary infiltration goes on in all porous rocks at enormous depths, and that the deeply seated walls of the volcanic ducts leading to the crater, if of such materials, may be red hot, and yet continue to pass water from every pore (like the walls of a well in chalk), which is flashed off into steam, and, unable to return by the way the water came down, escapes through the duct and crater. For my own part, I do not think this can happen during a state of undisturbed statical pressure, but that it follows on any disturbance.

That capillarity exercises a very important influence on the underground percolation of water is undoubted. To a certain point it has been proved experimentally by M. Daubrée,† who found that water placed on a disk of fine-grained (Triassic) sandstone,‡ fastened over a vessel filled with steam under pressure of nearly two atmospheres, infiltrated into the underlying vessel against that pressure. He further noticed that in consequence of the heat the action was more rapid than it otherwise would have been; and—making the experiment inversely—he observed that vapour placed under a pressure of several atmospheres in the lower vessel, did not transude through the disk left dry on the upper surface. As before pointed out, however, capillarity is adversely affected by a rise of temperature, and is comparatively inoperative at high temperatures.

Under these circumstances, it is conceivable that water may readily be carried down through the upper cooler strata to the proximity of the volcanic duct. But no amount of available vapour tension could force it back through the same depth of strata against both friction and capillarity. At the same time when the elastic tension of the vapour of the water reaches the point either of critical temperature, or such higher temperature that it exceeds the hydrostatic pressure, the further progress or descent of the water will be prevented. The influx of water to the volcanic duct is to a certain point effected under the same conditions as those which effect its general descent to depths through the earth's crust. But at this point, whatever it may be, other causes come into operation, which, while the descent of water to the *volcanic foci* beneath the solid crust remains an impossibility, renders its introduction into the *volcanic ducts*, even at considerable depths, possible. In any case, when an equilibrium is established between the vapour tension and the hydrostatic pressure, no change will take place unless that condition of equilibrium be disturbed. Such a cause exists in the case of a volcanic duct.

In fig. 7 the surface-waters pass in the usual manner through the strata *b* to some point *m*, where the heat from the lava L in the duct

* Palmieri's "Eruption of Vesuvius in 1872." Mallet's Introduction, p. 52.

† "Géologie Experimentale," 1879, p. 236.

‡ The absorbent power of this rock = 6·9 per cent. by weight.

gives the water an elastic force sufficient to bar its further progress, and fills the space *n* with vapour at an excessive tension, or with water at the critical point. This tension has determined a state of equilibrium which, so long as no change takes place in the opposing surfaces of the vapour and the lava, is maintained without change. If, then, anything occurs to weaken the resistance of one of the surfaces, and equilibrium be destroyed, the surface, if elastic, will yield. Thus, so long as a volcano is at rest, the pressure exercised by the column of lava L against the sides of the duct counterbalances the tension of the vapour at *n*. But, when the lava is forced upwards and flows past *n*, the statical pressure at that point is so far lessened that the tension of the vapour now exceeds the resistance of the lava at the same point, and the vapour is driven into the liquid lava L and rises with and through it to the surface. At the same time the sudden elastic expansion of the vapour lessens the pressure on the water at *m*, which therefore becomes in turn explosive, and as this water is replaced by a portion of that which is pressing forward from the water-head in *b*, this action will be repeated so long as the water supply lasts and the disturbing causes continue in operation.

The influx of water into the volcanic duct will also be materially influenced by the changes of level, or oscillation, which the column of lava undergoes during eruptions. For the variation of pressure caused by this state of changing equilibrium allows the vapour pressure to predominate at one time, while the resistance to it by the column of lava will be in excess at other times.

The explosion of steam into the lava involves, amongst other effects, disturbances of the encasing strata whenever any portion of the water or vapour explodes behind blocks of rock forming part of the walls of the duct. In fissile or porous sandstones, where the water cavities are small, it may be supposed that a minimum dislodgment of the materials composing the strata takes place. But if, instead of soft

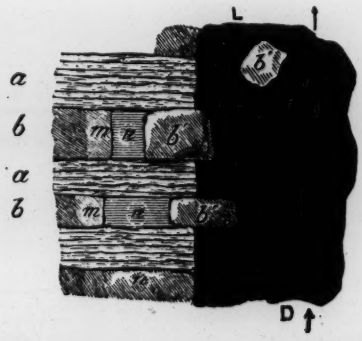


FIG. 8.—Projection of blocks from the permeable Strata *b* into the ascending lava (one side only is represented).

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* Fouqué, "Santori
† *Ibid.*, p. 12.

or incoherent sandstones, the strata consist of bedded compact limestone or other rocks, as represented in diagram, fig. 8, through which the water passes in open fissures, then the result will be different; for the rock being divided horizontally by the planes of bedding, and vertically by joints and cracks constituting the fissures in which the water is lodged, when the water surrounding the blocks b' explodes or flashes into steam they will be forced or blown out with the escaping steam into the lava current L, in which they will be carried to the surface and ejected with the lava-scoriæ by the explosions of the elastic vapour. The cavities formed by the expulsion of the blocks b' are immediately injected with the lava from column L which eventually consolidates there. At the next eruption, when the former conditions of water level have been restored, the renewed explosions at m n will expel not only other portions of the strata, but also portions of these intruded masses of lava. It is thus that in the earlier eruptions of Vesuvius, which form the slopes of Somma, blocks of altered limestone derived from the underlying Tertiary and Cretaceous strata are common; they are scarcer in the later eruptions. It was while forming the walls of the duct, and in contact with the molten lava and heated water or vapour, and not while imbedded in the lava, that these rock masses probably underwent that metamorphism which, in the case of the Vesuvian lavas, resulted in the formation of the very varied group of minerals met with in the ejected blocks of Somma. Similar metamorphosed blocks are found in the old lavas of many volcanoes (Santorin and others), whilst the blocks of quartz mica-schist occasionally met with have undergone little or no change.*

Besides these blocks of Sedimentary rocks, there are also found in the more recent lavas of many volcanoes blocks of lava having the character of the early erupted lavas. Fouqué mentions that in the eruption of Santorin in 1866, there were enclosed blocks of a lava of which the composition differed considerably from that of the flowing lava, and that they contained the minerals common in the old lavas of that island.† It is difficult on any other hypothesis to account for the presence of such blocks.

I consider, therefore, that a volcanic duct passing through a certain number of permeable strata charged with water, and through volcanic matter also charged with water below a certain level, is surrounded at these points by, as it were, an explosive material under repression, which only requires a disturbance of the equilibrium, under which it exists when the volcano is in a state of repose, to explode with violence. Further, as the superheated vapour from each point of contact forces its way into the ascending lava, the contiguous portion of highly heated water behind it is driven forward, replacing that which has

* Fouqué, "Santorin," p. 10.

† *Ibid.*, p. 12.

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12.

just escaped, while another portion of the water filling the stratum is pressed forward to take the place of the exploded portion. If the lava still continues to ascend, the in-forcing of the vapour and the explosions of successive portions of water succeed almost uninterruptedly, and the volcanic duct thus becomes the centre to a battery discharging at many levels and keeping up an incessant volley; for the supply of water at first is comparatively unlimited, and being renewed as quickly as it is exploded, there is no cessation in the action until the source is drained or stopped or the lava ceases to flow. If the water is lodged at such a distance, *n*, from the sides of the duct that it explodes behind blocks or fragments of rocks, or of older lava lodged in the cavities left by previous removals of portions of the rock, then such blocks will be blown and driven into the outflowing lava current. This seems to be a more probable explanation than that of their being torn or wrenched off the sides, although in the first stage of a new volcano there may often be great mechanical action and ejection of *débris* in clearing the passage, as in the case of the large quantity of *débris* of Devonian strata in so many of the old volcanoes of the Eifel.

In considering the various phases of this problem, it is, however, only too apparent that while the hydro-geological conditions admit of investigation on known principles, the thermo-dynamical conditions are involved in much obscurity, and are more hypothetical. The warrant for any hypothesis depends, however, upon whether the observed phenomena accord with the inferences that should follow on the assumptions, and for this I think sufficient cause can in this instance be shown. For the more or less deep-seated subterranean detonations and thundering that accompany most eruptions, and the paroxysmal explosions accompanied by enormous ejections of steam and ashes from the crater, are the necessary consequences of an influx of water into the volcanic duct under the conditions we have described; while a term is placed to the continuance of the eruption in the circumstance that the water supply being external and independent, whenever that supply is exhausted by expulsion through the escaping lava, the explosions must cease, although the eruption of lava may proceed for a longer period. The hypothesis agrees also with the fact that as a rule the eruptions are more paroxysmal the longer the interval of rest, for the filling of the underground reservoirs exhausted by the previous eruption is a question of time, and the greater their water-stores, the greater and more powerful the explosions.

The apparently conflicting phenomena of Ehrenberg's discovery of fresh-water diatoms in the volcanic ejections of some islands, and of marine diatoms in others, admit also of ready explanation on this hypothesis, for so long as the inland underground waters due to the

rainfall stand higher, which they must do whenever the land is even only a few feet above the sea level, so long will those waters dam back and keep out the sea-water, but whenever their level is brought below that of the sea level, then inevitably will the sea-water flow inland until the level is restored. Thus fresh-water remains accumulated during a period of repose, may be ejected during the early stages of the eruption, and may be succeeded by marine ejections when the exhaustion of the fresh-water springs leads to the influx of the sea.

There are difficulties in adapting this hypothesis to volcanoes in a state of permanent action such as Stromboli and Kilauea, but they are not more formidable than those presented on other hypotheses.

In both those volcanoes there is a maintained state of slight unstable equilibrium—a constant oscillation of the lava in the volcanic duct coupled with a small loss, whether in the form of ejected scoriæ or of slight occasional overflows of lava. As these conditions are permanent, they would induce in the manner before described a permanent influx of water either through permeable strata at depths, or by soakage of surface or sea water through the loose volcanic matter above; and as owing to this incessant drain, the underground reservoirs would not have time to fill to their full extent, so it is probable that the influx of water would always be small, but sufficient to maintain the slight and constant disengagement of vapour bubbles that goes on. Mallet says that there is a perennial spring on Stromboli at a higher level than the crater.

The independence of neighbouring volcanoes also presents another difficulty. If, however, the volcanoes of Southern Italy or those of Hawaii are, as suggested by Dana, on separate lines of fissure, it is possible that on the assumption of excessive viscidity of the lava, friction may oppose greater resistance along the horizontal planes which separate them at depths, than along the vertical column of ascent in which the increasing fluidity, as the pressure lessens, diminishes the friction. This is merely a suggestion. Although seemingly independent, there is occasionally both in the Italian and in the Hawaian volcanoes, symptoms of sympathetic action.

On the hypothesis advanced in these pages, a reason is also afforded why a greater volcanic activity should be maintained along coast lines than inland, in the circumstance that when the inland waters which feed for a time the explosive action are exhausted, they are succeeded through their now deserted channels, by an influx of sea water that serves to keep up for a longer period the state of volcanic activity, and maintain passages open for the extrusion of the lava. It is thus that the great inland volcanic areas of Auvergne, the Eifel, Hungary, and Central Asia have, on the withdrawal of the surrounding waters, become gradually extinct, and that the great volcanoes of the present day have settled in ocean centres, or along coast lines.

In mostly all of existing volcanoes, there is clear evidence of the access of sea water in the presence not only of the chlorides and other products of its decomposition in the emanations from the lava, but also in many cases of sea salt itself. Not, however, that the presence of sea water is necessary or is always present; for the vast stores of underground water in the Andes, and the great distance from and height above the sea of so many of the South American volcanoes, render it probable that they are in the main dependent upon inland water supplies, and this is confirmed by the researches of Boussingault, who was unable to detect in the fumaroles he examined any traces of chlorides.

But if water only plays the secondary though important part I have assigned to it, to what are we to attribute the motive power which causes the extravasation of the lava? This involves questions connected with the rigidity and thickness of the earth's crust, that we will proceed to consider in their geological bearings.

§ 6. *Thickness of the Earth's Crust from the Geological Standpoint.*

Geologists and physicists still hold from their different standpoints divergent views respecting the thickness to be assigned to the crust of the earth. Although the present stability of the earth's surface renders it evident that the hypothesis of a thin crust resting on a nucleus altogether molten and fluid is untenable, it is equally difficult to reconcile certain geological facts with the hypothesis of a globe solid throughout, or even of a very thick crust.

Nor have the phenomena of the tides yet been determined with sufficient accuracy to settle definitely the moot question whether the rigidity of the crust is perfect, or whether it yields to some very small extent to the deformation that might be caused by slight internal tides. Therefore, while on this ground alone, even if the data were not otherwise insufficient, there are, on the other hand, certain geological phenomena, volcanic phenomena among the rest, which are not only incompatible with an entirely solid globe, but which would seem to be explicable only on the hypothesis of a thin crust and a slowly yielding substratum. Of course it is also open to consideration, whether a crust and substratum of this nature would not, under certain conditions, offer sufficient resistance to produce a *quasi* rigidity such as would accord with existing physical conditions.

The phenomena on which on geological grounds I should chiefly rely in proof of such a substratum, and of a crust of no great thickness, are—

1st. The flexibility of the crust as exhibited (*a*) in the uplift of mountain chains, and (*b*) in the elevation of continental areas.

2nd. The rate of increase of temperature with the increase of depth from the surface.

3rd. The volcanic phenomena of the present day, and the welling-out of the vast sheets of trappean rocks during late geological periods.

a. It is important for our object to note that not only has mountain-uplifting gone on through all geological time, but that many, if not most, of the great mountain chains have been raised during the latest geological periods, and that compressed uplifts have not been confined to any limited district, but have extended over the several continents and over both hemispheres. As instances of these may be named:—

1. The elevation of the Pyrenees, which, although commenced in Palæozoic times, attained its maximum intensity and development in Oligocene, while minor movements continued to Miocene times.
2. The main elevations of the Rocky Mountains and portions of the Andes took place during the Tertiary period, and they were raised to their present height so late as in Miocene and Pliocene times.
3. Although considerable elevations of the Himalayas are of Pre-Tertiary date, the researches of the geological survey of India* show that the special great Himalayan disturbance is of Post-Eocene age; while in the Sub-Himalayan ranges, there is a large amount of disturbance of Post-Pliocene date.†
4. The elevation of the main axes of the Alps (although, like the others, began earlier) took place in Miocene times, and was prolonged to as late as the Post-Pliocene period, or to the time immediately preceding the comparatively recent Quaternary period.

It is only necessary to look at the section of any mountain chain to see the enormous amount of squeezing and crumpling the strata have invariably undergone, and the succession of folds of vast magnitude into which they have been thrown. In the Alps there are seven, if not more of these great folds, each constituting a mountain chain. In a straight line across they measure about 130 miles; but, if the strata were stretched out in the original planes, it is estimated that they would occupy a space of about 200 miles.

Le Conte states that the coast range of California consists of at least five anticlines, and as many synclines so closely compressed that a width of 15 to 18 miles of horizontal strata has been reduced to 6 miles.

* Medlicott and Blanford's "Geology of India," pp. 569, 570.

† It is a question even whether the earth movement along this great axis of elevation has yet wholly ceased.

These are common geological facts. I need add but one more instance on account of the magnitude of its scale.

Professor Clarence King, speaking of the plication of those parts of the Rocky Mountains which lie in Wahsatch and Uinta, estimates that the folds there measure 40,000 feet from summit to base.* What must have been the contraction in horizontal distance where the strata form not one but several folds, the crown of whose arches attain a height to be measured by miles!

It is difficult to see how these corrugations of the earth's crust are to be accounted for, unless we assume that the crust rests on a yielding substratum, and that it is of no great thickness. For if the earth were solid throughout, the tangential pressure would result not in distorting or crumpling, but in crushing and breaking. No such results are to be seen, and the strata have, down to the time of the youngest mountains, yielded, as only a free surface-plate could, to the deformation caused by lateral pressure. Freedom and independence of motion are evident in these wonderful contortions and inversions of the strata, and for that result a soft and yielding bed on which the crust could move as a separate body is necessary. Nor is evidence wanting that such a yielding plastic bed does exist, for rising up in the central axes are not unfrequently masses of crystalline rocks, which were then or shortly anterior in a viscid state, or else the strata are penetrated by dykes and veins of igneous rocks indicating a still greater fluidity of the broached fundamental base.

These facts are so patent to geologists that it may seem almost superfluous to adduce them. Let us suppose not entire solidity, but a crust 800 miles, or even half 800 miles thick. What would be the magnitude of a mountain chain resulting from the crumpling and upthrow of such a mass of rock? Where have we evidence in the latest of our mountain chains of the existence of such masses? Nowhere do the disturbed and tilted strata point to a mass more than a few miles thick, for the whole of the sedimentary and metamorphic rocks are often uptilted, together with a portion of the molten rocks on which they rested. Had the crust had the more excessive thickness suggested by physicists and by some geologists, we should have had mountains if not of greater height, at all events of greater breadth; for if a solid plate of any kind be broken and the fractured edges turned up by reciprocal pressure in presence of a viscid resisting material beneath, the width of the protruding mass will bear a definite relation to the thickness of the plate. If, on the other hand, the plate is sufficiently pliable to yield without fracture and be bent into folds, the height of the arch and the width across the fold will in like manner be proportionate to the thickness of the plate.

* "Geology of the 40th Parallel," vol. i, p. 761.

Where have we evidence, even in the most recent of mountain chains, when the earth was approaching its present conditions of rigidity, of a shell of 700, or 500, or 100, or even of 50 miles thick? Would it not rather appear that a crust of 30 miles is even in excess of what the height and breadth of any mountain chain would, on this finding, indicate?

To the first part of the argument, it may be rejoined that the existence of a thinner crust and of a fluid nucleus is not contested in the various geological periods, and that the conditions of solidity and rigidity are only applicable to the globe as it at present exists. But the observation loses its point when we consider that the cooling must have been slow and gradual throughout all time,—that the formation of mountain chains has been intermittent with long intervals,—that the last-formed chains show no change in the character or diminution in the forces to which they are due,—and that there is nothing to indicate such a sudden accession of solidity in the earth as would be involved by the assumption of the free play of the crust during Tertiary times and its entire rigidity now; whilst, on the other hand, other forms of the forces coordinate in a cooling globe still continue in visible action. If one form of a force dependent on a common cause remains in operation, we are scarcely justified in assuming that another consequence of the same cause, though dormant, is extinct. Our limited experience suffices to make us acquainted with the more persistent effects, but fails to compass those which are intermittent.

b. With respect to the other analogous forces in operation now and in times immediately preceding our own, they likewise indicate a yielding substratum—although the disturbances at present caused by them may not result in the fractures and contortions involved in mountain-forming, and which are only the effects of prolonged and extreme tension. I allude to those wide-spread movements which result in great superficial or continental upheavals—movements which, of frequent occurrence in all geological times, have not altogether ceased in our own times. Our object is, however, with the later periods only. It will be sufficient if we go back towards the close of the Tertiary period.

We have merely to look at a geological map of the world to see that a very large portion of the existing continents have been under the sea during the Tertiary period. South-eastern England, a large portion of France, great part of Spain and Italy, and the whole of Central Europe, have, since the Eocene period, undergone movements of elevation *en masse* with little disturbance to the strata over large areas, and these have been prolonged down to Miocene and Pliocene times. In the same way the elevation above the sea of great part of Asia Minor and Mesopotamia is of Post-Miocene age. Nor has the

yielding of the crust during this late geological epoch been confined to these limits. Great parts of India, Australia, and of the seaboard of North and South America, have been under the sea and raised at various intervals during the whole of the Tertiary period.

But it is not these movements—great and general as they were—that so immediately concern the question before us. Our object is to show that the flexibility of the crust, which is exhibited in all geological periods, has been continued without break and over large areas down to the latest period, and that the older changes link on to changes in progress in our own times—changes admitting of a measurement which enables us to realise their importance.

The presence of shells of recent species at certain elevations in Central England and Wales prove that those areas have undergone an elevation of not less than 1400 to 1500 feet after the inset of the Glacial period. Ireland and Scotland have undergone changes little less in the same period; and so likewise has the North American continent. The rock of Gibraltar has been raised 1400 feet or more at even a later period. The *massif* of Scandinavia, the long coast-lines of the Pacific side of South America, have been raised 500 or 600 feet or more during the life of existing species of Mollusca. Over portions of the Pacific basin islands have been raised 200 to 300 feet or more, and the coasts of Arctic America and Asia exhibit conclusive evidence of similar recent elevation. These movements of elevation, which bring us down to the threshold of the present times, link on without break in the latter areas (and many others might be named), with the changes of level which some of those areas are still undergoing.

Changes of level have been ascribed by some to the change of temperature at depths, caused by the shifting upwards of the underground isotherms, in proportion as the strata have increased in thickness by the successive addition of fresh sediment. This, as suggested by Babbage and Herschel, may be a true cause under certain conditions of thick accumulation of strata, but must necessarily, as the expansion of rocks by heat is so small, be limited to moderate vertical displacement, and fails to explain the greater changes of level to which we have just referred, for neither in the Old nor in the New World do the later Tertiary strata exceed, as a rule, a thickness of 1000 to 3000 or 4000 feet, so that the difference of temperature in the strata so covered up would not usually exceed 20° to 100° F., and would rarely attain to a rise 150° F. Supposing this to affect an underlying mass of rock, say 100,000 feet in depth, the effect would be to increase its dimensions vertically from about 10 to 100 feet only. This also is on the assumption that the underlying mass of rock remains stationary; but if, whilst the accumulation of strata proceeded, the area itself, as has usually been the case, subsided, the only effect of expansion of the

mass, unless it all took place at once after the deposition of the overlying strata was ended, which is impossible, would be *pro tanto* to diminish the rate and extent of subsidence. Further, alterations of level arising from this cause are not likely to have taken place in Glacial and Post-Glacial times, as the sedimentary matter then deposited was generally limited to a few irregular masses of sand, gravel, and shingle, or to a few raised beaches and shell beds only a few feet in thickness; while the submarine beds rarely reached a thickness of more than 300 or 400 feet.

The next point we have to consider is, that of the increase of temperature with the increase of depth. On this, there is a general agreement as to the fact, but a difference of opinion as to the rate of increase and in the inferences to be drawn from the observations. The rate of increase is found to differ very materially at different places, varying in round numbers from 30 to 100 feet for each degree F., or with a mean by some taken at 50 and by others at 60 feet; and conclusions have been drawn by some geologists from these observations that the nucleus of the earth is in a state of fluidity at a depth of 30 miles; whereas others have considered the facts not incompatible with a crust of 800 miles or more thick.

The variation in the rate of increase with depth is influenced not only by the variable conductivity of the strata, but depends also upon so many disturbing causes, that while all the observations tend to prove the general fact of an increase of temperature with depth, very few can be relied upon to give an exact measure of that increase. Notwithstanding the care taken by some of the earlier and all the later observers against these disturbing causes, these are so numerous and often so difficult, if not impossible to guard against, that only a limited number of observations can be relied upon to give the exact data required. I have gone elsewhere* into the various considerations affecting this question, so that they need not be repeated here. The conclusion that I have arrived at is, that the observations that can be best relied on show an increase of not less than 1° F. for every 48 feet of depth, and which there is some reason to believe may be even more rapid.

Whether or not the rate of increase diminishes or increases with the depth is a question which requires further investigation. The few observations that have been made throw little light on the subject. These are given in the paper before referred to. They certainly do not tend to prove that there is any diminution in the rate of increase of temperature. There are some reasons on the contrary which would lead to an opposite opinion.

* *Ante*, p. 1.

The received conductivity of chalk, sand, clay, and some sandstones, through which so many of the bore-holes and shafts in which the observations have been made have been carried, is low. It has been inferred in consequence that the isothermal lines of depth are closer together in these rocks than they would be in the underlying crystalline and igneous rocks, and therefore that these surface rocks give us a more rapid rate of increase than would be found to obtain at greater depths. But it must be remembered that in general the experiments on the conductivity of rocks have been made with specimens carefully dried; whereas all underground rocks hold a certain quantity of water, the water of imbibition; and further below a certain level—that of the line of water-level—they are charged with a further quantity of water, or the water of saturation. Every bore-hole or shaft has to do with rocks under these conditions. Now, the experiments of Messrs. Herschel and Lebour show that there is a remarkable difference between the conductivity of rocks in their dry and wet state. Thus, taking the rocks just named, these differences are as under, k being the thermal conductivity, and r the thermal resistance.

	Dry.		Wet.	
	k.	r.	k.	r.
Quartzose sand.....	0·00105	952	0·00820	122
Clay	0·00250	400	0·00370	270
New Red Sandstone..	0·00250	400	0·00600	166 ?

It appears by this that sand, which is one of the worst conductors when dry, becomes one of the best when wet, being only exceeded by rock salt, vein quartz, and quartzite; while the average conductivity of the three substances above named exceeds when wet (k=0·00597, r=186) that of many igneous rocks. The average of five common varieties of the latter gives k=0·0524 and r=196; with five varieties of granite the average is k=0·00584 and r=173, and taking together several varieties of Palæozoic limestones k=0·00572 and r=177.

Experiments on wet chalk, oolite, and coal-measure sandstones are wanting, but as the water of imbibition of these several rocks is as under, it is to be inferred that when wet their conductivity would be raised in proportion with that of the others:—

	Per cent. of water.		Per cent.
Coal shale	2·85	Chalk.....	24·10
Sandstone..	4·37 to 13·15	Cal. freestone.	16·25

On the other hand, the harder crystalline and igneous rocks imbibe but little water:—

Granite	0·06 to 0·12	per cent.
Devonian limestone	0·08	„
Basalt	0·33	„
Slate	0·19	„
Quartzite	0·66	„

Then, again, the conductivity, even when dry, of the "ganister" and other hard Coal-measure sandstones is found to be higher than that of either the metamorphic or igneous rocks, averaging for four varieties $k=0{\cdot}00737$, $r=136$.

It seems, therefore, probable that the unaltered sedimentary strata are often, under their normal underground conditions, as good, if not better, conductors of heat than the crystalline and igneous rocks which constitute the greater proportion of the solid crust of the earth, and, if so, the thermometric gradient would rather tend to become more rapid in passing from the former into the latter than otherwise.

But there is another cause which when the subject comes to be more fully investigated will have to be considered—that is, the change of conductivity produced by heat. Forbes found that the conductivity of iron varied with the temperature as under:—

0° C.	0·0133
100 „	0·0107
200 „	0·0082

This shows a percentage decrement of the decrease of conductivity of iron between 0° and 100° C. of 24·5, which agrees nearly, according to Professor Tait, with the empirical law that the conductivity is inversely as the temperature. Looking at other physical properties which the metals and rocks have in common, it is not improbable that they may have also this other property, although, no doubt, in a very modified form. It is the more probable, inasmuch as the proportion of iron present in the igneous rocks (commonly 10 to 15 per cent.) is larger in those which are supposed to be the more deeply seated; while considering how largely the density of the mass of the earth is in excess of the density of the crust, there is reason to believe that the proportion of the metals increases with the depth. This would produce an important effect on the thermometric gradient.

A large number of the observations on temperatures at depths have been conducted in carboniferous strata consisting largely of sandstones of high conductivity; and others through chalk, clays, and sands of which the conductivity becomes high when wet,—and wet they must be at even small depths beneath the surface. On the other hand, the crystalline and igneous rocks are so hard and compact and absorb so little water, that their conductivity can be little influenced by this cause.

After eliminating those observations where the results are affected by some of the various causes of interference, it would appear that the sedimentary rocks *in situ* do not possess a lesser power of conduction than the igneous and crystalline rocks which underlie them, so that the rate of increase of temperature need not on this account be less rapid. This is especially a conclusion which may be drawn from the deep (4172 English feet) and remarkable boring of Sperenberg, where the bore-hole first passed through not quite 300 feet of gypsum, and then entirely through rock salt of which the conductivity is extremely high (0·0128), exceeding that of any other rock; nevertheless, the rate of increase is 51·5 feet per degree F.* Had it been a rock of lesser conductivity the rate of increase could hardly be otherwise than more rapid.

Assuming a uniform rate of 1° F. for every 48 or 50 feet of depth, the heat at a depth of 28 to 30 miles would be such as to fuse the basic rocks, and this has often been taken as a measure of the probable thickness of the earth's crust. But there is the uncertainty just named whether the rate may not increase, while the phenomena before named indicate that this even is too great a thickness to be in accordance with observed geological facts?

May it not also be a question whether, as I have before suggested, the intense cold of the Glacial period has not so affected the outer layers of the earth's crust that to a certain depth the rate of cooling is now abnormally slow, owing to the excessive refrigeration the shell then underwent. If that be the case, would not the rate of increase of temperature at greater depths be more rapid than that which our observations on the chilled layers have led us to assume, so that the thickness of the crust might be even less than the 28 to 30 miles just named? Such a conclusion would be more in harmony with geological phenomena, as we shall proceed further to show.

The third objection to a thick crust is the difficulty of reconciling such a condition of things with the effects of volcanic action. This branch of the subject may be divided into the phenomena connected—1stly, with recent volcanoes; 2ndly, with the great outwellings of trappean rocks during the later Tertiary period; and, 3rdly, with the character of the changes of level in the areas so affected.

The first point relates especially to the apparent impossibility of a column of lava traversing a crust 800 to 1000 miles thick in consequence of the enormous pressure required, and without the loss of so much heat in such a length of passage as to cause the lava to lose its fluidity and consolidate before it could reach the surface.

To meet this difficulty Mr. Hopkins suggested that the solid crust contained at various depths beneath the surface cavities filled with

* "Brit. Assoc. Reports" for 1876.

fluid incandescent matter, either entirely insulated or perhaps communicating in some cases by obstructed channels, and that in these subterranean molten lakes the volcanic foci originate.*

To this view it is to be objected that the variation in depth from the surface, and the existence of separate molten lakes is not compatible with the singular uniformity as a rule of the volcanic rocks over the whole globe; again, lakes would be required co-extensive with large continental areas, and therefore there seems no object in the limitation; while further on this hypothesis there would seem to be no available cause for the extrusion of the lava other (but applying with greater force) than that assigned by Mr. Scrope, the objections to which I have already named.

Again, the enormous outwellings of trappean and volcanic rocks which took place at intervals during the Tertiary period and continued down to Quaternary times, afford evidence of the existence of a fluid magma underlying the solid crust, co-extensive not only with the existing volcanic outbursts, but also with these older eruptions, and spread the volcanic phenomena over areas so large and so numerous that it is difficult to conceive their isolation as separate and independent local igneous centres.†

In this country the great basaltic plateau of the North of Ireland is 600 to 800 feet thick, and extends over an area of about 1000 square miles; those of Western Scotland are of about the same extent, and it is certain that both had, before the coast denudations, a much wider range. In Central France there is a still wider basaltic area of yet more recent date. There are others of great extent in Hungary and in Central Italy. They cover also large tracts in Asia Minor, Africa, New Zealand, Australia, and America. But to confine ourselves to two instances on a grand scale we may take the great plateaux of Central India and of North-west America.

In India these plateaux stretch for a distance of 500 to 600 miles from north to south, and 300 to 400 miles from east to west, covering, according to the reports of the Indian Survey, an enormous area of not less than 200,000 square miles.‡ They have a general thickness of from 2000 to 3000 feet, and it is estimated that the total thickness of all the beds amounts to not less than 7000 feet. They are of late Cretaceous or early Eocene date, and consist of a succession of beds spread, no doubt, over a long period of time.

In North America vast sheets of basaltic rocks form the high plateau of Utah, while on the Pacific slopes immense regions have

* *Op. cit.*, p. 54.

† See also the Address of Sir Wm. Thomson, in Section A, Brit. Assoc., 1876, in which the evidence regarding the physical condition of the earth is reviewed.

‡ "Manual of the Geology of India."

been flooded by outpourings from fissures at successive times from the close of the Miocene down to the Quaternary period. In Columbia these basaltic rocks have a thickness of from 1000 to 3000 feet, and in parts of Colorado of not less than 4000 feet, and they stretch over a tract some 700 to 800 miles in length by 80 to 150 miles in width, and cover 120,000 to 150,000 square miles of surface.

Extensive as are the ejections of some volcanoes at the present day, and vast as are some of the individual lava streams, the sum total is small compared with these older extrusions. Lyell instances as amongst the most remarkable of the modern lava streams that formed during the eruption of Skaptán Jokul, in Iceland, in 1783. He states that it formed two branches of the relative lengths of 50 and 45 miles, and of the extreme breadths of from 12 to 15 miles, and of 7. Taking the mean breadth, we have an area of 500 square miles covered by the lava of this eruption, with an ordinary thickness of about 100 feet, and an occasional one, when it filled gorges, of 600 feet.

But we have no means of judging of the single flows of geological times; we can only take the total areas covered by recent volcanic eruptions, and compare them with the old basaltic outpourings. The two most extensive modern volcanic surfaces are those of Hawaii and Iceland. The thickness of the masses at the centre of eruption probably exceeds, though the average of the whole mass is certainly below, the thickness of the basaltic beds in the cases named above, but the areas covered by the volcanic materials show very different measures. The area of Hawaii is about 3800 square miles and is entirely volcanic, and that of Iceland is 37,800 square miles, of which the volcanic beds are said to occupy about one-third to one-half. At the outside, therefore, the modern eruptions are spread over no area larger than 20,000 square miles, or an area only equal to one-tenth and one-seventh of the old Indian and American basaltic areas.

Now, if these vast erupted masses had been derived from local molten lakes of moderate size and moderate depths, the extravasation must have caused a diminution in their masses which, as the loss could not be made good by drafts from adjoining areas, must necessarily have led to a caving in and subsidence of the crust above these lakes proportionate to the mass of the extravasated matter. But so far from this being the case, the areas of these great basaltic outwellings are almost always areas of elevation. The basaltic area of the Deccan forms vast plateaux which attain a height of between 4000 to 5000 feet, and although the intercalated sedimentary strata are mostly of land and freshwater origin, there is reason to believe from the circumstances that on the borders of the same district some of the associated beds contain estuarine remains, that the area was immediately prior to the eruption not much above the sea-level.

In America also the basaltic plateaux rise gradually to heights of from 2000, 3000, and 4000 feet, and in some cases even attain the height of 11,000 feet or more. Similar evidence, though on a much smaller scale, is afforded by the basaltic plateaux of Ireland and Scotland, of Central France, and of other countries which form relatively to the surrounding districts more or less elevated tablelands, raised above the sea-level mostly in Tertiary and many in very late Tertiary times.

It is impossible to attribute the elevation of these vast flattened domes to any secondary causes, such as expansion by heat of strata undergoing subsidence by transmission of the isothermals. The difference of level is, as before explained, too great. Besides, the crust in these areas must on the whole, so far from gaining in any part, suffer a considerable loss of heat by the very circumstance that the heat brought by the lava to the surface is lost by radiation. There would, therefore, be every cause for depression of the crust were the molten lakes local and independent. These areas of eruption are, on the contrary, areas of elevation—not as in mountain chains by lateral squeezing and an upward thrust along narrow anticlinal lines, but by elevation *en masse* of wide portions of the earth's crust possibly accompanied by fracture but without, necessarily, contortion.

We have, therefore, in the discharge of volcanic matter *primâ facie* evidence of the existence of molten matter beneath the surface, and, in the domed elevation of the surface, of a yielding substratum, fluid or viscid, underlying the solid strata. Further, it follows from the fact of the upheaval that the igneous rock ejected is not only replaced, but that it is replaced by a quantity larger than that which is lost by extravasation. This could only be effected by supplies from adjacent areas of similar matter—in other words, it indicates that there must be a common fluid or viscid substratum, yielding to depression in some areas, and to upheaval in others, the loss in the one case being counterbalanced by an addition and centralisation in others. Apart from the great movements which raised the basaltic area of the Deccan, Dr. Blanford* states that in the Indian Peninsula there is evidence bringing down movements of elevation to the extent of 100 to 200 feet to so late a period as the old raised beaches (of Pleistocene age) that, on the other hand, the presence of the Maldive, Laccadive, and Chagos groups of atolls and coral reefs in the sea to the south-west, points to slow depression; and that there is unmistakeable proof of a recent sinking of the land on the Arabian coast near the mouth of the Persian Gulf. There is evidence also of recent depression in the Delta of the Ganges and of the Mississippi, and probably in that of the Indus.

Though attended with more uncertainty, there is reason to believe,

* "Geology of India," pp. 376 and 378.

as suggested by Darwin, that great coral areas of the Indian and Pacific Oceans have long been areas of subsidence,* while adjoining volcanic areas have been areas of elevation. In some cases areas, once areas of depression, have become areas of elevation, as in the instances of some coral islands, which, though formed during periods of depression, have been since raised above the waters to heights of from 200 to 300 feet or more.

In conclusion, I may point to the imposing spectacle afforded by the slow secular upheaval of the vast tracts of Arctic lands on the shores of North America and Asia—an area of elevation so extensive that it embraces almost all the land bordering the Polar Seas. This elevation has in comparatively modern times raised the land from 100 to 400 feet above the present sea-level, and is still, in our own times, in visible action over a superficial area extending in some directions, for thousands of miles.

§ 7. *Primary Cause of Volcanic Action.*

If water only plays a secondary part in volcanic action, and the presence of the vapour of water in the volcanic foci be not the primary cause of the expulsion of the lava, to what other cause is it to be attributed? I see none but a modification of the old hypothesis, namely, that of the contraction of solid crust upon a yielding and hot nucleus. The objection to this hypothesis rested mainly on the fact that if, as was at that time assumed, the whole nucleus beneath the solid crust consisted of a molten fluid, it would be subject to tides that would lessen or neutralise the surface tides. I might also suggest as a further reason that there could be no local deformations on a continental scale:—the pressure caused by local squeezings would be dissipated through the entire mass and lost. Sir William Thomson and the late Mr. Hopkins have clearly proved that the earth possesses a rigidity perfectly incompatible with a fluid nucleus. At the same time, objections have been taken by other physicists to the hypothesis of an entirely solid globe† on the grounds, amongst others, that the question has been dealt with on the assumption of a perfect fluid, and that not sufficient allowance has been made for friction.

Nor are those views so incompatible as might at first sight appear. The questions connected with the surface tides are not yet settled. Admitting an extreme rigidity of the crust, it has not yet been proved that notwithstanding this rigidity, the crust is quite free

* This has been contested. There are no doubt instances of reefs formed without subsidence, but for the depths of the Pacific Darwin's hypothesis best answers all the conditions.

† See the various papers on this subject by Hennessy, Haughton, Delaunay, and others.

from the influence of some slight disturbances—sufficient, however, for the purposes of our contention.

For the geological argument, neither a perfectly fluid substratum nor a molten nucleus are required. The hypothesis of a central solid nucleus seems to me to be the only one compatible with geological phenomena. All that is required for the conditions of geological phenomena is that on this solid nucleus there should be a molten yielding envelope—not fluid, but viscid or plastic; nor is it necessary that it should be of any great thickness, but a thin crust is, geologically, an essential condition. The relative proportions of the two are, however, questions for physicists. The late M. Roche did attempt a solution based on the astronomical and physical conditions of the problem. Assuming the earth to consist of a solid centre with a density of about 7·0, and of an outer layer consisting of a fluid substratum with a solid crust, and having a mean density of 3·0, he found that the outer layers should have a maximum thickness equal to one-sixth of the earth's radius, or 660 miles, but he left the question of their possible minimum thickness open to other considerations.

The considerations I have already urged, in conjunction with the results of other independent physical investigations, would lead me to assign lesser dimensions to this combined thickness of the outer layers. On geological grounds the solid crust at all events need not have a thickness of even 20 miles; while, on the same grounds, the dimensions required for the underlying molten layer to place it in concordant relation with the ascertained mobility of the crust during the later geological periods, is that it should be a mass sufficiently large for the play of movements such as would come within the compass of continental (not mountain) elevations and depressions; and for this object and for the purposes of vulcanicity a molten layer, having a thickness measured not by hundreds, but by tens of miles, would fulfil the necessary conditions.

It is quite possible, as suggested by Scrope, that owing to pressure the fusion-point of lava at great depths is so much higher than at the surface, that the lava may, and possibly does, exist at depths in a viscid or plastic state, and only becomes fluid as it rises to the surface and the pressure is removed. This state of viscidity accords with the excessively slow rate of movement and steadiness of the great continental elevations and depressions,—changes in close relation one with another, and which may arise from the slow transference from one area to another of a partially resisting plastic medium within confined limits.

I cannot conceive such a transference to be effected unless that the molten layer were of moderate thickness, so that when locally compressed between the outer solid crust and the inner solid nucleus, that portion of the magma subjected to pressure would expand laterally

and the mass displaced would be transferred to that part of the adjacent area where the outer crust would yield most readily to deformation and upheaval. If the matter displaced were not confined between resisting surfaces, or if the molten layer were of indefinite thickness or of extreme fluidity, the effect would not be local, but would extend as far as the liquidity of the mass allowed of the transmission of the displaced matter, so that it would tend to become attenuated and lost in the larger volume of which it became part. The result is, in fact, a condition dependent on the measure of viscidity.

A compression in one part should, therefore, be followed by expansion in another, and by a deformation of the crust over conterminous areas. These effects are exhibited in the great continental upheavals and depressions so rife in the times immediately preceding our own, and still in a measure of perceptible action over a large part of the world; as, for example, in the instance of the slow uplifting of the northern portions of the Scandinavian and Greenland peninsulas, and the subsidence of their southern portions, while further south again in Labrador the land has been rising.

This constitutes the essential difference between the disturbances connected with mountain- and volcano-forming. They both result from the contraction due to secular refrigeration, but the one is a process of excessive lateral compression, and the other of turgid swelling of the crust, for all active volcanoes are on raised sedimentary strata. In both cases there is tension, although of a different character. In the latter it is slow, steady, and uniform in its action, and where there are permanent points of comparatively slight resistance, as in volcanic ducts, it then readily finds relief in the expulsion of the lava, which is only prolonged until the equilibrium is restored; then the eruption ceases and the volcano lapses into a state of rest, only to be broken when again there has been an accumulation of the necessary energy.

The agency of water is confined to the secondary effects I have described—effects perfectly independent of the forces which produce the extravasation of the lava; and while, with the thinner crust of former times, there would be a more frequent extrusion of the igneous rocks, there is probably, with the thicker crust of the present day and its greater resistance, larger stores of underground water and greater explosive eruptions.

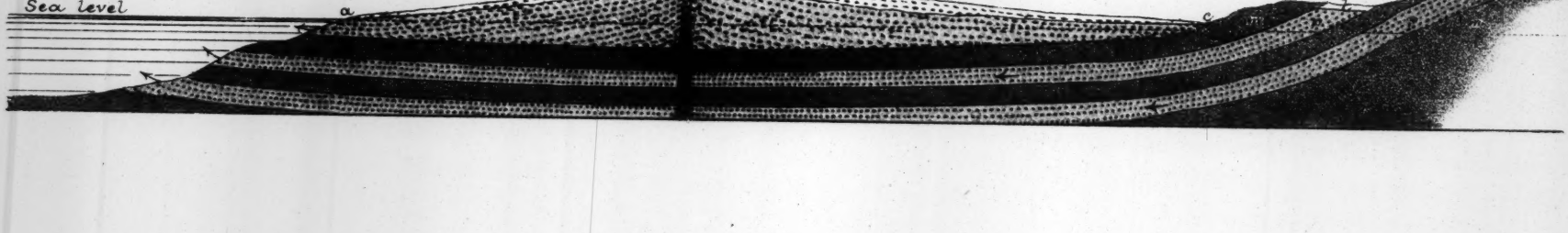
In connexion with this subject, it is a noticeable fact that volcanoes, although not entirely confined to a definite zone, are far more numerous within 50° lat. N. and S. than beyond those limits, while in the northern hemisphere there are, with one exception, none beyond the Arctic circle; whereas it is beyond that circle, and where we may suppose the crust of the earth to be thickest, that the great continental movements of upheaval are now most active and maintained.

Proc. Roy. Soc. Vol. 41, P.L.1.

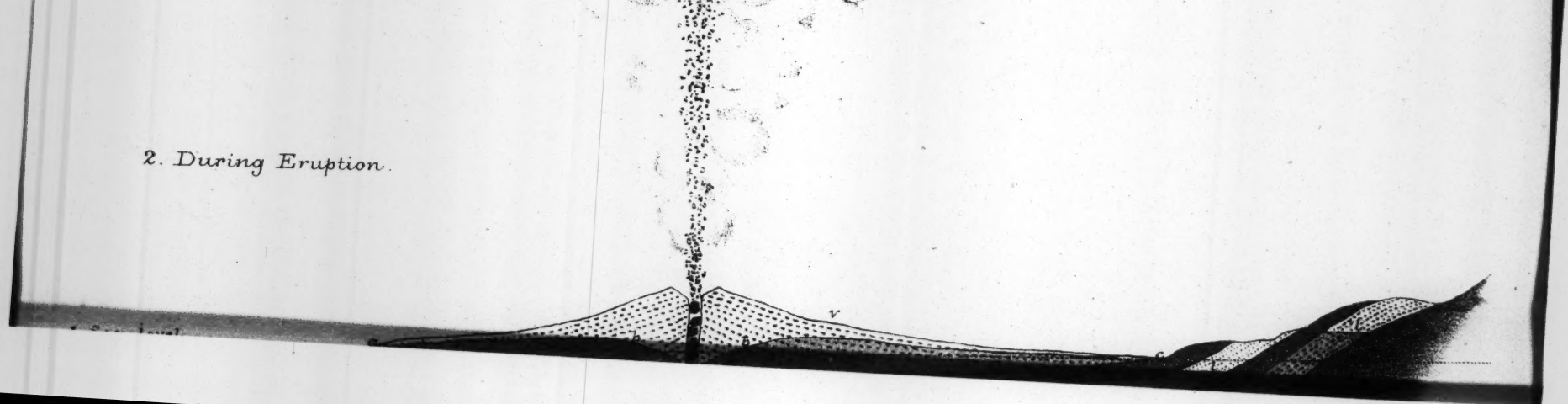
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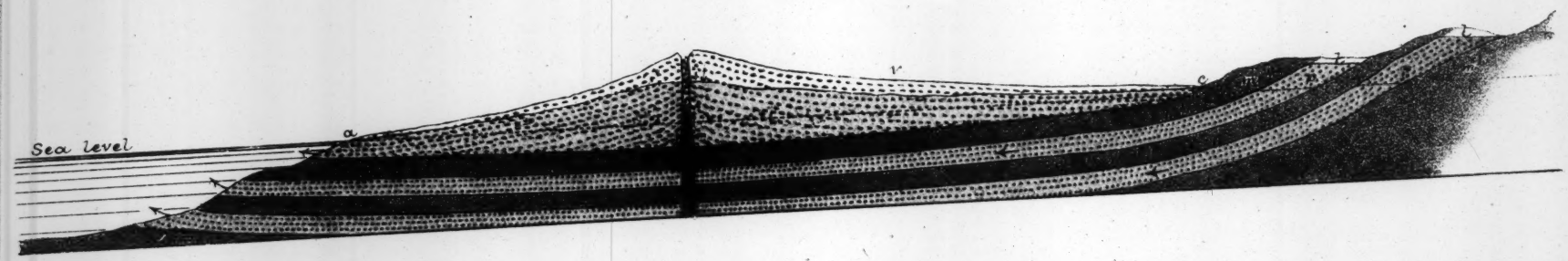
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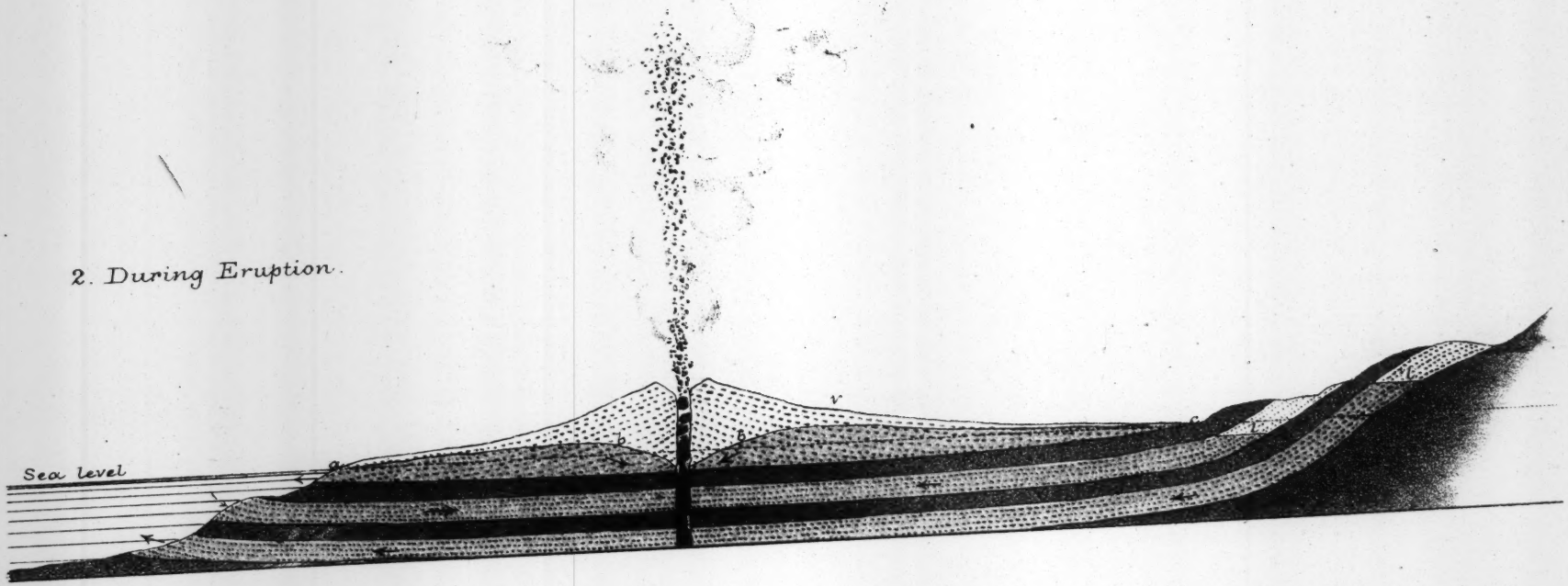
2. During Eruption.



1. Before Eruption.



2. During Eruption.



3. *After Eruption*

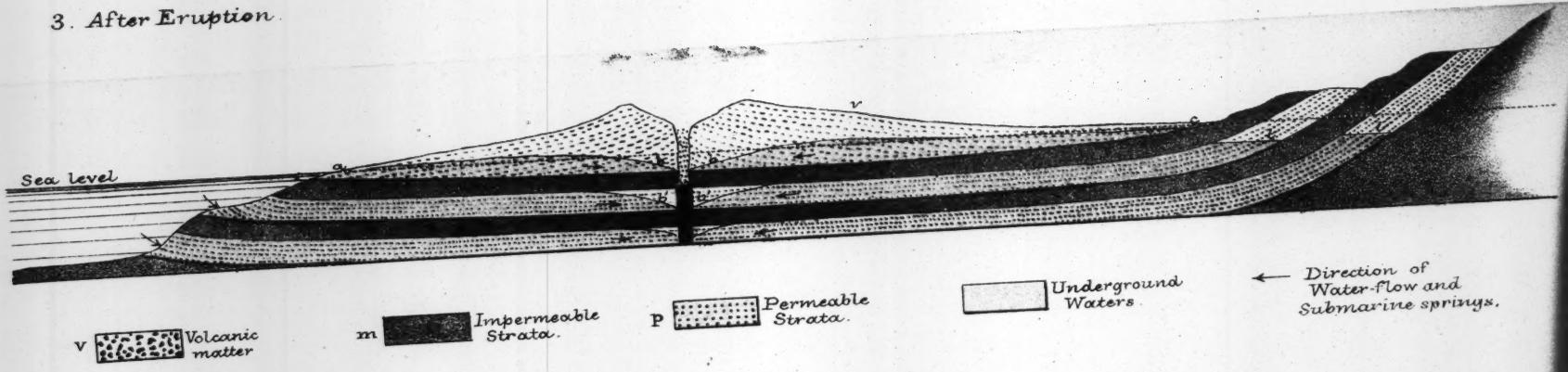
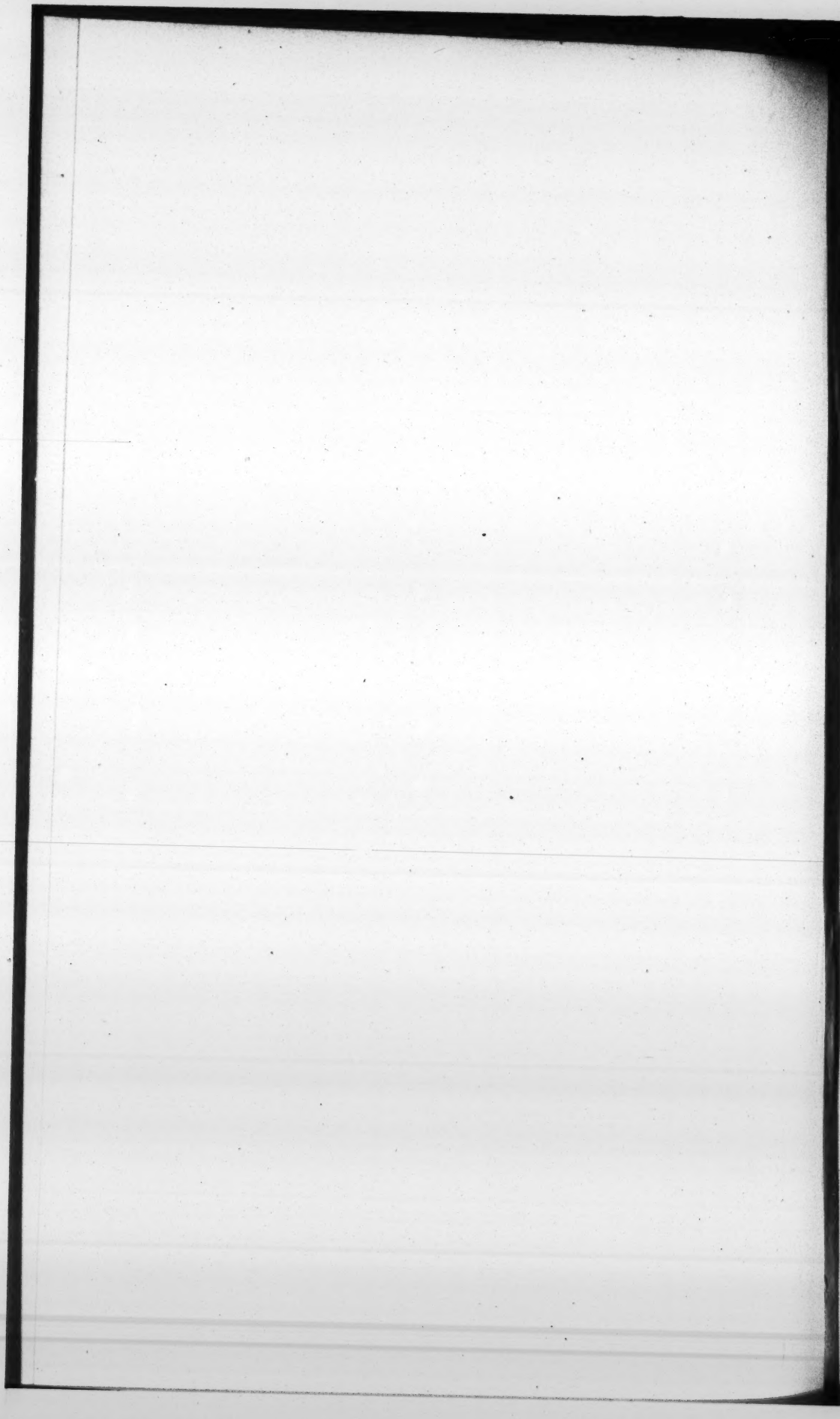


DIAGRAM – SECTIONS OF A VOLCANIC ERUPTION.

West Newman & Co lith.



The loss of terrestrial heat by radiation is now exceedingly small. But small as this loss is, it cannot take place without producing contraction, and Cordier long since calculated that supposing five volcanic eruptions to take place annually, it would take a century to eject so much lava as would shorten the radius of the earth to the extent of 1 mm., or about $\frac{1}{25}$ inch.

I therefore conclude that the hypothesis originally propounded, namely, that volcanic phenomena are dependent on the effect of secular refrigeration is, with certain modifications, the one that best meets the necessities of the problem.

"Preliminary Report on the Pathology of Cholera Asiatica (as observed in Spain, 1885)." By C. S. Roy, F.R.S., J. Graham Brown, M.D., &c., and C. S. Sherrington, M.B. Received and read June 10, 1886.

The differences of opinion among pathologists as to the relation of certain micro-organisms,—and more especially of a curved bacillus described by Koch—to Cholera Asiatica, led to our being deputed last summer, by the Association for the Promotion of Research in Medicine, acting in conjunction with the Royal Society and University of Cambridge, to proceed to Spain to make further investigations on the subject.

In Madrid we were able to make autopsies on twenty-five typical cases of cholera, the *post-mortem* examinations being made in many of them either immediately after death or within very few hours of it. Our attention was at first directed chiefly to the relation of the comma bacillus of Koch to Cholera Asiatica. Early in our inquiries we were struck by the fact that Koch's comma bacillus is not discoverable in the intestinal contents of all cases of cholera. We employed much time and care in the examination of thin dried films of the mucous flakes and fluid contained in the intestine, these films being stained by methods which we knew to be well fitted to show the comma bacillus if that micro-organism were present. Such films from one or two fatal cholera cases showed that the intestinal contents contained enormously larger numbers of comma bacilli than of other parasites. In some of the cases the comma bacillus, though certainly present, was not the most marked feature in the preparation, and in certain of these cases we were only able to find these bacilli after prolonged and careful searching. In many cases of undoubted Cholera Asiatica where death occurred before the reaction stage had set in, we were unable to detect comma bacilli in any of the films or cultures prepared from the intestinal contents taken from different parts of the alimentary canal.

We made also an enormous number of plate cultivations in gelatine and agar-agar, the cultivating media employed being prepared in the manner recommended by Koch. These cultures gave results corresponding with those obtained from examination of the dried films above referred to. In a few cases the majority of the colonies which grew in the gelatine were composed of comma bacilli. In others these were comparatively few in number, while in many of our plate cultures from the intestinal contents no colonies of comma bacilli presented themselves. The results both of the examination of films and of the plate cultures from the contents of the intestines are therefore opposed to the facts obtained by Koch. We do not think that the complete absence of the comma bacillus in the films and cultures of many of our cases can be explained by want of due care on our part.

When we came to the preparation and examination of sections of the wall of the intestine, the results we obtained were also different from those described by Koch. The tissues intended for histological examination were placed in absolute alcohol while the autopsy was being made, and the staining method which we chiefly employed was that used by Koch with methylene-blue, which method in our experience is well fitted to show the comma bacillus when it is present in the tissues.

Only in a few of our cases did we find the bacillus in question situated in the substance of the mucous membrane of the intestine, and in these cases it was present either in or close to the tubular glands of Lieberkühn or else close to the free surface of the mucous membrane. The situation of the bacilli in these cases was such as led us to the belief that it might easily have penetrated the epithelium either after death or in the few hours preceding death. In the great majority of our cases careful and conscientious search did not enable us to discover any comma bacilli in the mucous membrane of the intestine, nor indeed in any of the tissues or organs which we examined. Under these circumstances we did not think it necessary to make special investigations on the pathogenic effects of the comma bacillus when administered to the lower animals. We have not been able to convince ourselves that the pathogenic effects which Koch, Van Ermengem, and others find to result when comma bacilli are given to certain animals are identical with the phenomena which characterise Cholera Asiatica in man.

Further, the complete absence of comma bacilli, both in the contents of the intestine and in the tissue of many of our cases, makes it impossible for us to accept Koch's views as to the causal relation of the bacillus in question with Cholera Asiatica.

At the same time we do not think that sufficient evidence has been adduced to prove satisfactorily that Koch's comma bacillus is identical

either with that described by Finkler and Prior as having been obtained from cases of Cholera nostras, or with those other forms of curved bacilli which have been found in the mouths of healthy persons, decayed teeth, cheese, &c. That it is pathogenic scarcely admits of doubt, in view of the evidence given by Koch and V. Ermengem. The view, however, that it is to be looked upon as the direct cause of cholera is, we think, incompatible with the results we obtained, as referred to above. Many points of similarity between Koch's and Finkler and Prior's comma bacillus, and the relation between the latter and Cholera nostras, have led us to consider the possibility of the former of the two being the cause of the so-called premonitory diarrhœa which is so frequently, if not constantly found accompanying epidemics of Asiatic cholera. The frequency with which the comma bacillus of Koch is to be found in cases of cholera makes it, we think, almost certain that it has some relation to the disease in question. That the so-called premonitory diarrhœa of cholera cannot be looked upon as a mild form of Asiatic cholera we feel convinced from our own observations in Spain. At the same time we think it must be looked upon as a predisposing cause of Asiatic cholera. It seems to us by no means improbable that Koch's comma bacillus may be the cause of this so-called premonitory diarrhœa, and that its frequent presence in fatal cholera cases may be due to true cholera having supervened. Unfortunately while in Spain our researches were not sufficiently complete to enable us to decide that Koch's comma bacillus could not be looked upon as the direct cause of cholera, and our attention, therefore, was not directed to the question of its being found in the dejecta of persons suffering from the so-called premonitory diarrhœa. This is a question which we believe well worthy of further investigation.

As to the straight bacilli obtained by Emmerich from the blood and organs of fatal cholera cases, our results, by use of similar methods to those employed by Emmerich, do not enable us to confirm those of that observer. The cultivating media in which we placed drops of blood, or portions of the kidney, liver, and other organs from cholera cases, remained sterile. We cannot, therefore, accept the bacillus of Emmerich as the cause of Cholera Asiatica, and see no difficulty in explaining the pathogenic effects which he observed on introduction of his bacillus into certain of the lower animals as being due to a form of Septicæmia.

The straight bacillus described by Klein in his Report of the Indian Cholera Commission, is much smaller than that found by Emmerich; and we know of no reason for believing the two to be identical. As Klein has not claimed for the bacillus found by him any causal relation to cholera, we need only say here that we have not been able to recognise it in any of our preparations.

The pleomorphism of the comma bacillus which Ferran described in his article in the "Zeitschr. f. Klin. Med." of last year, and the method of inoculation for protection against cholera, largely employed in Spain, attracted our attention while in that country. We had an opportunity of examining with the microscope the cultures sent, at the request of the Government, from Ferran's laboratory, to be employed in inoculating troops. These cultures were under the care of agents duly authorised by Ferran. We found them to contain several forms of bacilli and micrococci, but we did not discover either comma bacilli or any of the peronospora-like bodies described by Ferran to be present in his protective inoculation fluid. We need only say further that the facts which came to our knowledge regarding the protective power of Ferran's inoculations are by no means in accordance with the statements made by that observer. The inoculation of such fluids as we saw in the hands of Ferran's agents must expose the recipient to serious peril of septicæmic infection. The theory, moreover, on which Ferran's system is based, *e.g.*, that one attack of cholera protects against a second attack of the same disease, is entirely opposed to the facts which came under our notice in Spain. As, however, we did not personally meet with Dr. Ferran, we have no right to affirm that the cultivations when they left his hands were impure; we believe indeed that the small swellings described by him in cultivating the comma bacillus under certain conditions—and which are found in the course of the spirillum—rest upon correct enough observations. They have been seen by others, and are apparently degenerative changes. Large peronospora forms described by Ferran we knew of no reason to accept as being possible modifications of the comma bacillus.

With regard to the macro- and micro-scopic morbid changes to be found in fatal cholera cases, our observations are completely in accord with the classical description given by Strauss, Nocard, Roux, and Thuiller. With regard to the comma bacillus, the observations of Strauss, in which he was unable to satisfy himself of the causal relation between the comma bacillus of Koch and Asiatic cholera are also fully accordant with our own investigations.

Although specially commissioned to investigate the relation between Koch's comma bacillus and Asiatic cholera, we naturally turned our attention to the pathology of the disease as well as to its etiology.

That the hypersecretion from the mucous membrane of the stomach and intestines in cases of cholera is not due to catarrh, is abundantly shown both by the chemical characters of the dejecta of cholera and by the absence in the mucous membrane, on examination after death, of the anatomical changes which occur in catarrh. That it is not due to any irritant present in the contents of the intestine we think may safely be concluded from the absence of necrosis (ulceration), as well as the absence of any well-marked inflammatory

change in the mucosa. The experiments of Moreau, who found well-marked hypersecretion of the mucous membrane of the small intestine, resulting from section of its nerves, seems to us to have a very important bearing on the pathology of the disease in question. The chemical characters of the fluid so abundantly secreted during the few first hours following section of the nerves of the isolated loop of intestine are, as shown by Kühne, practically identical with those of the dejecta of cholera cases. That the hypersecretion from the intestine in cholera, with the more or less complete arrest of the power of absorption by the mucosa, is the cause of the thickening of the blood and the drying of the tissues there can be little doubt. Everything seems to point towards the assumption that the chief symptoms and signs of cholera, as well as the frequent fatal termination of that disease, is due to the escape of fluid from the vessels into the intestine. The pathology of cholera is therefore centred in the question as to what is the cause of this intestinal secretion and diminished power of absorption. We have repeated Moreau's experiments more than once with the view of observing for ourselves how far the fluid contained in the isolated loop of the intestine resembles the intestinal contents of fatal cholera cases. The results of these experiments are such as to impress us strongly with the conviction that the characters of the paralytic secretion following section of the nerves to the intestine are practically identical with those of the contents of the intestine in cases of cholera. With regard to the question whether or not the epithelium covering the free surface of the mucosa is simply less firmly attached than in the normal intestine, or whether it be actually detached before death, our observations tend on the whole towards the first-named of the two possibilities.

The pathology of cholera, we are of opinion, can be best explained by some cause acting on the glands or nerves of the intestine, and producing effects similar to those which result from paralysis of the intestinal nerves. This conclusion makes it probable that the cause of the disease, if discoverable by the microscope, is to be found either in the mucosa itself, in the nerves running in the mesentery, or in the ganglia from which those nerves proceed.

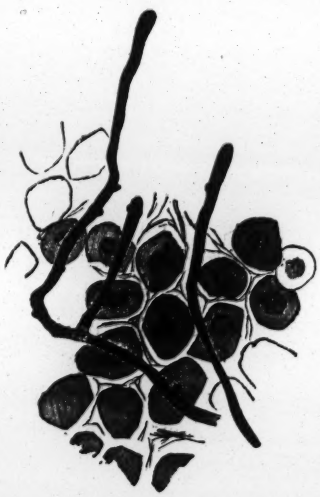
Soon after our return from Spain our inability to confirm Koch's views as to the causal relation of the comma bacillus to cholera induced us to look with care for any other parasite, the presence of which might be supposed capable of causing the disease in question. In the course of this work, which occupied many months, we observed in sections of the intestinal wall, prepared and stained in the different ways which we thought best fitted to make evident any micro-organisms present in the tissue, granules the characters of which arrested our attention. These granules, which vary much in size, were for the most part smaller than the nuclei of the surrounding

cells—which they resembled in the degree to which they were stained with fuchsine, methylene-blue, and other aniline dyes—that they had one or more processes proceeding from them, we were always able to observe; but these processes, being unstained, could not be followed by us in the tissues. It was only where in a specimen of a kidney we saw several of these granules lying in the lymph space outside the membrana propria of a convoluted tubule that we could see against the clear background the arrangement of the filaments which we had noticed proceed from the granules in other specimens. We could then see that delicate filaments connected the granules one with another in a manner so striking that the possibility was forced on us of the structure in question being a vegetal parasite. As we ourselves know no more regarding the morphology of the lower vegetal micro-organisms than is known ordinarily to the pathologist, we judged it best to show the specimen to Mr. Vines and Mr. Gardiner, whom we thought well fitted to assist us in the question whether or no we had before us a vegetal parasite, and if so of what kind. They at once informed us that forms such as the one we showed them were characteristic of the Chytridiaceæ, an order which includes many rapidly growing and virulent parasites of vegetables. They informed us, moreover, that they should not expect the envelope of such parasites to give the ordinary cellulose reactions; and also that in their opinion some method of staining in methylene-blue would be found best fitted to differentiate such a micro-organism from the surrounding tissues. The necessity which we felt of finding some method which would stain the mycelium-like threads of this structure before its presence could be demonstrated in the tissues of cholera cases, led us to spend much time experimenting with the different methods of staining in methylene-blue which we thought might be of use for this purpose. After many failures, we found that it was possible, by using a method almost identical with that of Löffler, to stain with methylene-blue the micro-organism in question, although even with this method considerable care is required to stain satisfactorily the filaments of which it is partly composed. When observed in a successfully stained preparation, it is impossible to doubt that the structures referred to are parasitic, which will, we believe, be admitted on examination of the specimens which we show at this meeting. The difficulty of staining the parasite satisfactorily, and the fact that, so far as we know, Löffler's method has not been employed to stain sections of tissues taken from fatal cholera cases, sufficiently explains to our minds why a parasite having so striking a form could have been overlooked by the many competent observers who have sought for micro-parasites in the tissues of fatal cholera cases. That its presence in our specimens could not be explained by accidental contamination after removing the tissues from the body we have satisfied ourselves by

most careful precautions. During the autopsy pieces of the intestinal wall and of the other organs are placed in absolute alcohol, and all the fluids through which the tissue and sections required to be passed for the purpose of cutting, staining, &c., well boiled and protected from the invasion of vegetable organisms by boiling and the addition of thymol.

The micro-organism which we find in our specimens from cholera cases is composed of terminal or nodal swellings connected by filaments varying greatly in thickness, and showing no differentiation into component cells.

FIG. 1.



Mucous membrane of small intestine from Case XIV, showing a portion of deep part of mucosa (Powell and Lealand $\frac{1}{12}$ oil immersion).

FIG. 2.



Similar section from Case XXIV ($\frac{1}{12}$ oil immersion).

Frequently also we have seen exceedingly fine filaments extending from the granules, like those which form so marked a characteristic of many of the Chytridiaceæ. Stained with methylene-blue, the granules and filaments take on the colouring matter to a fairly equal degree, although with fuchsine, followed by diluted nitric acid, the filaments become decolorised before the granules. The globular enlargements referred to vary in size from knot-like swellings on the mycelium, causing simply an inequality in the thickness of the latter, to bodies as large and in some cases even larger than the nuclei of the surrounding cells. In this abstract, in which we cannot give satisfactory illustrations, we need not say more regarding the morphology of the parasite than that it differs considerably in appearance in different cases, always, however, within the limits included in the above description, and that there are frequently present minute, round,

unstained and stained spaces or bodies in the granules and mycelium, which suggest the possibility of spores being present. The development of zoosporangia from the terminal granules, such as are described in some forms of the Chytridiaceæ, we have never seen in our specimens. In every one of our twenty-five cases of Asiatic cholera we have found a parasite having the above-described characters lying in the mucous membrane of the intestine. It is found at various depths of the mucosa, its distance from the free surface, the absence of other micro-organisms, and the precautions taken to prevent post-mortem contamination, make it evident that it must have been present there during life. We find, moreover, that the luxuriance of its growth goes, speaking very roughly, hand in hand with the degree of the histological changes in the structure of the mucous membrane, which are found unequally distributed in the mucosa of the small intestine in fatal cholera cases. In some of our cases we have found it in or between the epithelial cells of the tubular glands of the intestine.

As to its presence in other organs and tissues, we have satisfied ourselves of its presence in the kidney in some of our cases. That it occurs in the blood-vessels in some cases we have also convinced ourselves, and think it possible that the filaments and bodies described by Strauss as growing from the blood corpuscles may have been the micro-organism which is above described.

The time required for the successful staining of this micro-organism has prevented us as yet from making a systematic examination of the kidney and other organs in all of our cholera cases, so that we are not at present in a position to say in what organs besides the mucous membrane of the small intestine it is frequently or constantly present. We have seen it, however, both in the liver and kidneys, as well as in the intestinal wall. As to the question whether this parasite can be cultivated artificially, our observations are unfortunately very unsatisfactory. While in Spain we made a very large number of cultivations from the contents of the intestine, from the blood and tissues, but found it impossible to investigate the life history of all the micro-parasites which we encountered. Our *à priori* belief was that the parasitic cause of cholera, if it exist (which we thought exceedingly probable), must belong to the class of the Schizomycetes. Our attention, therefore, was almost exclusively directed to the bacilli, bacteria, and micrococci, which grew in our plate cultivations, other micro-organisms receiving little or no attention. We may, very possibly, have grown the parasite which we afterwards found in the wall of the intestine, passing it over, however, as being a very improbable cause of the disease. It appears to us by no means necessary that this micro-organism will grow readily in the alkaline cultivating media which Koch found best suited for his

comma bacillus, and we would suggest to future observers that possibly acid media may be found better suited for the growth of this parasite. As to whether the micro-organism is frequently or constantly present in the contents of the intestine, our cultivations as above-mentioned are of no value. Nor does our examination of stained films enable us to speak definitely for or against the presence of this parasite. In the intestinal contents or the dejecta of cholera cases, we have seen appearances which resembled in many points the characters of the micro-organism in question; none of our films, however, show structures which are *unmistakeably* identical with those found by us in the substance of the mucous membrane.

We have only to add that not having cultivated this micro-organism artificially, we claim no right to say that it is the cause of Asiatic cholera. All we have to say is, that we have found it present in certain tissues of all the cholera cases which we had the opportunity of examining. Further investigation must decide whether or not it can be looked upon as the direct cause of the disease.

"An Instrument for the Speedy Volumetric Determination of Carbonic Acid." By WILLIAM MARCET, M.D., F.R.S. Received June 9. Read June 10. Revised by the Author June 29, 1886.

[PLATE 2.]

The principle of this instrument is the absorption of carbonic acid in a closed receiver by potassium hydrate, and the accurate measurement of the volume of dry atmospheric air required to re-establish the atmospheric pressure after complete absorption. The volume of air used for that purpose will exactly correspond to that of the carbonic acid gas absorbed. It is obvious that whatever be the reading of the barometer, the volume of air corresponding to that of the carbonic acid absorbed, will give the correct proportion of carbonic acid in the air submitted to analysis; but to obtain the weight of the gas present, and its proportion by weight, it will be necessary to reduce both the volumes of air analysed and of carbonic acid found to their volume at 0° (C.), and under a pressure of 760 mm. of mercury. Hence the necessity of recording the height of the barometer at the time of the experiment, or one reading may suffice for a number of determinations.

The instrument resembles two small gasometers, and consists of two tanks and two bell-jars, or air-holders, each of the latter being made to hold half a cubic foot of air. The bell-jars hang on a metallic cord which connects them with each other, and passes over two pulleys,

allowing the bell-jars to be moved up and down alternately in the tanks. The inside of the bell-jars communicates with the outside air by means of a U-shaped iron pipe, one limb of which opens inside the bell-jar above the fluid it contains, and the other outside on a somewhat lower level for sake of convenience. The tanks or troughs contain glycerine, over which floats a layer of almond oil an inch or two in thickness, and filling approximately the trough. The opening of the pipe above the surface of the oil enters loosely, so as to leave free passage of air, a neck in the bell-jar admitting a thermometer fitted air-tight through it. By this arrangement when the bell-jar is lowered, the thermometer descends into the inner limb of the U-pipe or stand-pipe without dipping into the oil or glycerine.

A measured volume of air to be analysed is drawn into one of the air-holders (selected for that purpose) by raising it to a certain height, and on lowering it slowly, after cutting off communication with the outside by a stop-cock, the air is driven first of all through an absorption apparatus and then into the second bell-jar, which has at that time an ascending motion. The whole apparatus is connected by brass tubes and stop-cocks, on one hand with a pressure-gauge, and on the other hand with a graduated glass receiver, immersed in mercury, and fixed to a bracket and rack movement by which it can be raised and lowered at will. The various parts of the instrument are all in air-tight communication with each other while the carbonic acid is being absorbed, so that the absorption will tell on the pressure-gauge, and by depressing the measured glass receiver into the mercury trough, and driving the air it contains into the air-holders, while keeping the eyes fixed on the pressure-gauge, it will be easy to restore the atmospheric pressure in the whole apparatus, and thus determine the volume of gas absorbed.

I shall now beg to describe in succession the various parts of this instrument, and next state how the analysis is made.

The Tanks.—One of the great difficulties I met with was the selection of the fluid to be used in the tanks. Water would not answer, first of all because it absorbed carbonic acid, and next on account of the tension of aqueous vapour. I tried a nearly saturated solution of common salt in water, so as to avoid any loss of carbonic acid by absorption, but after experimenting for a considerable time I had to reject the plan because of the vapour-tension, which I found impossible to take into account. I came to the conclusion that it was necessary to dry the air and submit it to analysis in the dry state, keeping it entirely out of reach of aqueous or other vapours. Bearing this in mind, glycerine appeared to me to be possessed of the required properties as a medium for confining the air in the bell-jars, and therefore for filling the tanks. It had the advantage of holding no water, but its thickness appeared at first sight a serious objection, as

from its adhesion to the sides of the bell-jars on their being raised, I thought there would be a difficulty in obtaining a fixed volume of air within them. However, glycerine was tried, and the objection found to be less than I had imagined, but glycerine absorbed water from the air, and in due time would give it out to the dry air submitted to analysis, so that glycerine could not be used without a protection from atmospheric moisture. It then occurred to me that some kind of oil might be made to float on the glycerine, thus completely isolating it from the external air, and almond oil—which does not thicken by exposure to the atmosphere—was adopted. The bath of glycerine covered with almond oil in which the bell-jars are now immersed was prepared in October last ; there is about one hundredweight of glycerine in each tank, and I tested the glycerine of one of the tanks for water four months and a half after it had been, it may be said, in daily use. This was effected by placing a weighed quantity of the glycerine for about half an hour under a bell-jar over sulphuric acid, and after that time it was found to have lost absolutely no weight. The layer of almond oil had, therefore, perfectly answered its object. The oil for confining the air to be analysed, ensured the absence of any vapour-tension, or any possible loss of gas from absorption, and, moreover, by lubricating the bell-jars prevented the glycerine from adhering to them, the oil itself, from its lightness, running rapidly down the sides of the receivers on their being raised or depressed. A simple experiment demonstrates the action of the oil. Let a glass tube opened at both ends be closed at one extremity with india-rubber tubing and a pinch-cock, and then glycerine be poured into the tube. By pressing on the tubing the glycerine is driven up, and on releasing the pressure it slowly subsides adhering to the glass; but if a little almond oil be poured over the glycerine, on compressing the tube the glycerine and oil are raised, but on releasing the pressure the glycerine runs down quickly to its former position without adhering to the glass.

The bell-jars or air-holders gave me no little trouble. They were made at first of thin sheet iron, but this, after much labour, was found inadequate, as the changes of temperature of the external air affected the gas through the metal, too rapidly to allow of a sufficiently fine correction. Every possible contrivance was adopted to overcome this difficulty. The tanks and bell-jars were enclosed in a wooden case with glazed doors opening sideways, and tinfoil shields were placed round the bell-jars so as to shelter them from any heat radiated from the outside, but these arrangements were too complicated and the shields had to be given up. I came to the conclusion that the only plan would be to keep the bell-jars under water during the whole of the analysis, and this was done by making use of jacketed bell-jars, the space inside the jackets being filled with water. A completely new instrument was then

constructed, the bell-jars being jacketed and the inside space of nearly half an inch in thickness was filled with water. Later on I substituted for water a solution of sodium sulphate saturated at a temperature exceeding that of my laboratory, so that the annular space in the bell-jars became filled with a crystallised solution of the salt. I thought by this plan to ensure a perfect stability of temperature in the air of the bell-jars during the analysis, any increase of temperature outside being transformed into motion through its action on the crystallised mass. The result was nearly as I had anticipated; still, after trying the plan for a considerable time, I finally gave it up and returned to the water-jacket which I now find quite satisfactory. There is a scale to each of the bell-jars, divided into four equal parts, and a pointer fixed to the rim of the tanks; by bringing the same line on the scale opposite the pointer before and after the analysis, the operator will be certain that the bell-jars hold exactly the same volume of air in both cases, admitting of course that the atmospheric pressure has been perfectly re-established.

The jacketed air-holders are raised and lowered by means of an iron weight worked over pulleys by a rack and drum movement, and handle; when the two bell-jars are at the same height, or level, they may be entirely released of the weights without their position being disturbed; but if one of the receivers, say the left-hand one, is to be depressed from that position and emptied of the air it contains, the left hand weight is brought to bear upon it, and by a slow movement of the handle that receiver is immersed entirely in the bath while the other one is raised as high as it will go. When thus raised it draws in common air; this air is filtered through marbles or fragments of pumice-stone, moistened with a solution of potassium hydrate, and then through calcium chloride, so that no carbonic acid nor moisture be admitted into the bell-jar. It is, of course, necessary to keep the taps communicating with the external air open in both tanks when the bell-jar is being emptied, or the oil will be drawn into one of the stand-pipes. Should such an accident occur, which never happens with a little practice, the oil will have to be let out from a small aperture closed with a screw tap in the lower bend of the U-pipe, while the thermometer has to be removed from the bell-jar, letting in air through the neck in which it fits. It is better to return an equal volume of glycerine to the tank rather than oil, from the difficulty the latter would have in finding its level in the tank. Should oil be added the bell-jar will have to be raised so far as it can go, and time allowed for the oil to assume its level below the rim of the receiver.

The cord which connects the bell-jars is liable to expand or contract, but this is remedied by turning a screw, placed so as to shorten or lengthen the cord, while the eye is kept on the pointers of both tanks

when they are made to point to the corresponding lines on the scale. Should the pointer of one bell-jar meet say the middle line on the scale, and the middle line on the scale of the other be above the corresponding pointer, that bell-jar will have to be depressed by working the screw until the middle line on its scale also corresponds with the pointer. This can be done without any trouble, and has seldom to be repeated the same day, while, at times, days elapse in succession without necessitating this correction.

The Absorption-tube.—Various descriptions of absorption-tube were tried before I obtained it in its present form. It has to fulfil two conditions, viz.: it must first of all absorb all the carbonic acid, secondly give out no water. My present arrangement, which works very well, is as follows: a straight hollow cylinder, 2 feet 3 inches in height, made of thin sheet copper and silvered inside, is jacketed and surrounded with glycerine over which floats a thin layer of oil to prevent the absorption of water. The cylinder is filled with fragments of pumice-stone in the midst of which is inserted a long metallic tube. The cylinder is closed with an india-rubber cork, perforated, through which is inserted a glass T-piece inverted. A very delicate thermometer divided into tenths of a degree Fahrenheit is run through the upper tube of the T-piece into which it fits air-tight, while the horizontal limb is placed in communication with the left-hand bell-jar by means of india-rubber tubing. The thermometer has a very long stem, and its bulb reaches far down into the metallic tube in the midst of the pumice-stone, thus registering the temperature of the air in the absorption-tube. The space left between the fragments of pumice-stone measures 1600 c.c. A narrow U-shaped glass tube connects the copper cylinder with another wider vertical glass tube, the latter being filled with pieces of caustic potash, and into it a narrow glass tube is fitted air-tight with an india-rubber stopper; the latter is jacketed, the annular space being filled with water. Thus the heat formed by the combination of the carbonic acid with the alkali distributes itself throughout the absorption-tube, and is prevented from reaching the right-hand bell-jar by the water in the jacket which absorbs it on its way. The U-shaped tube connecting the vertical portions of the absorption apparatus has a tube soldered to its lower part and widened into a bulb, an arrangement the object of which is to drain out the alkaline fluid after it has done flowing over the fragments of pumice-stone; the tube beyond the bulb is finally closed with india-rubber tubing and a pinch-cock.

The alkaline fluid for the absorption of the carbonic acid consists of half its volume of a saturated aqueous solution of potassium hydrate, and half its volume of pure glycerine, and the mixture is made to dissolve as much caustic potash in sticks as it will take up. An excess of the alkali is invariably left in the fluid, so that

should any water accidentally reach the solution it will always remain saturated. This solution answered its requirements in every way. The thickness of the glycerine enabled it to adhere to the fragments of pumice-stone in the form of a thick layer, while the water of the potassic solution was so firmly retained that none was given out to the current of air passing through it. Placed over sulphuric acid under a receiver a sample of this mixture underwent no loss of weight. This fluid absorbs carbonic acid with the greatest speed, and I am not yet able to say when its power becomes exhausted. It is held in bottles kept near the absorption-tubes, and before each analysis about 200 c.c of the solution is poured into the copper cylinder, when it trickles down the pumice-stone appearing shortly afterwards in the exit tube, when it is let out into the bottle. The bulb blown on the exit tube is indispensable as a means of collecting the solution which remains behind and would otherwise stop the communication between the bell-jars—an accident which frequently happened until the present arrangement was adopted. The air after leaving the absorption apparatus just described passes over pieces of caustic potash, where it deposits any last traces of carbonic acid which may have escaped from the absorbing fluid; it is finally cooled by the water-jacket, and in this state is collected in the right-hand bell-jar.

The heat produced by the combination of the carbonic acid with the alkali introduced, was at first a serious difficulty in the working of my instrument. Various means were tried either to get rid of it or estimate its influence so as to correct it by calculation. At last I succeeded—I may say beyond my expectations—in doing away entirely with this difficulty. The means I employed was surrounding the absorption-tube with the non-conducting material glycerine, and determining the temperature of the air in this tube with a very delicate thermometer.

The measuring apparatus consists of a graduated receiver, holding about 800 c.c., and moving up and down over mercury contained in a strong glass trough. The receiver is drawn out at the top into a long tubular neck, and this is fixed by a bracket to a rack and pinion movement. The opening in the neck is connected by india-rubber tubing and a combination of T-pieces, with a gauge on one hand and the bell-jars on the other, while a small tube of calcium chloride is interposed in such a way as to dry the atmospheric air as it is drawn into the graduated glass receiver.

The gauge is a very pretty instrument, which was made for me at the works for the construction of physical instruments at Geneva. It consists of a glass U-tube which can be raised or depressed by a rack and pinion movement, and a web stretched across the tube, though fixed to an independent brass-piece. By this simple arrangement it is

easy to ascertain when the gauge is exactly under atmospheric pressure. The fluid I have selected for the gauge is coloured petroleum, on which the slightest difference of pressure will tell.

Finally, there is an arrangement of brass tubes and stopcocks through which the air, after having been driven from the left-hand bell-jar into the other through the absorption-tube, can be transferred from the right to the left bell-jar without retracing its passage through the absorption apparatus.

In my early experiments, india-rubber tubing, pinch-cocks, and glass T-pieces were used, but I substituted for these the present brass tubes and stopcocks, and the air-tightness of all the apparatus can be thoroughly relied upon. It is not possible to do away entirely with india-rubber tubes, but those in use are now varnished with several coats of copal varnish, and made thereby perfectly air-tight.

The Analysis.—The air to be analysed, which, so far has been air expired from the lungs or such air mixed with common air, is first of all collected in an india-rubber bag, faced on both sides with oil-silk in order to prevent any loss of carbonic acid by diffusion through its substance. The bag is next connected by india-rubber tubing with a gas-holder or counterpoised bell-jar, working in and out of a bath of glycerine covered with a layer of almond oil. By weighting the counterpoise of the bell-jar, the latter is slowly raised, aspiring the air from the bag. A scale on the bell-jar and a pointer on the rim of the tank enable the volume of air to be read off, but of course the extra weight used for giving the receiver its ascending motion will have to be removed and the holder placed under atmospheric pressure previous to taking the observation. I now find it more convenient to read off the volume of air to be analysed from a smaller bell-jar holding 11 litres of air. After collecting the air from the bag into the larger receiver, which, when full, contains about 42 litres, it is driven into the smaller bell-jar through a glass desiccator full of calcium chloride, where it leaves its moisture without losing any of its carbonic acid. In order to dry the gas thoroughly, it should pass through the desiccator at the rate of no more than a litre per minute.

The next step will be to bring this air in the small holder exactly under atmospheric pressure, and a special contrivance had to be adopted to effect this object. The instrument used is a clamp and a screw in connexion with each other; the screw, by means of a crank movement, raises or depresses the clamp, while the fulcrum is the rim of the tank to which this instrument is fixed. The cord holding the counterpoise passes through the open jaws of the clamp, and is free to move up and down; but when the air in the receiver has to be brought under atmospheric pressure, by turning a nut the clamp is closed and the cord fixed: then, on moving the screw with another nut either to one side or the other, the bell-jar is raised or

depressed. The adjustment is exceedingly fine, and by testing the pressure of the air in the holder with a gauge supplied with coloured petroleum, the air in the bell-jar can be brought under atmospheric pressure with the greatest degree of accuracy. I am indebted to Mr. W. Parkinson, Engineer, for the mechanism of this delicate adjusting movement.

The (smaller) air-holder is now connected with the left-hand bell-jar of the analysing apparatus by varnished india-rubber tubing, in the track of which is another tube of calcium chloride (omitted in the plate), care having been taken to rinse out previously this connecting tube with the air to be analysed. The bell-jar is now raised by turning the handle of the drum and lifting slowly the iron weight which keeps the receiver immersed in the tank. When filled, the air is brought under atmospheric pressure by the adjusting apparatus, and a minute or two are allowed to elapse to make sure that the pressure is constant; the volume of air to be analysed is then read off on the scale of the air-holder.

The temperature indicated by the thermometers in the two bell-jars is observed by means of two little telescopes fitted into the glazed shutters of the case, and which magnify considerably the divisions of the instruments. By this means the temperature in the bell-jars can be estimated to the fiftieth of a degree Fahrenheit, and are recorded when steady; the temperature in the absorption-tube is also noted. After opening and closing the requisite stopcocks, the air is driven slowly by means of the drum-handle through the absorbing apparatus into the right-hand bell-jar, which is simultaneously raised. I find from experience that about 7 litres are sufficient, and indeed the most convenient for the analysis of air expired from the lungs; and four minutes suffice for the absorption of the whole of the carbonic acid of that air run over the alkaline solution. After the first operation, it is obvious that the air filling the pipes of the left-hand bell-jar has been left behind. By means of a combination of tubes and taps, the two bell-jars are now brought into immediate communication, and the air is returned into the left bell-jar up to the original mark on the scale without passing through the absorption-tubes. Meanwhile, a considerable suction has taken place in the apparatus from the absorption of the carbonic acid, and this is shown by the pressure-gauge. The small graduated glass receiver, originally full of common air under atmospheric pressure, and which stood at 0, is depressed into the mercury, and by this means atmospheric pressure is readily re-established in the whole apparatus, as seen by the petroleum in the gauge returning to exactly the same level in the two limbs; the carbonic acid normally present in this atmospheric air is not taken into account as too small in its proportion to affect the correctness of the

result. It may be advisable to drive the air into the apparatus by degrees as the absorption takes place, and this is my ordinary mode of procedure. In order to ensure the absorption of the whole of the carbonic acid, leaving behind but an insignificant fraction, the air should be passed at least three times through the absorption-tube, or, in other words, should be made to circulate three times through the apparatus before recording the total amount of carbonic acid present. The small fraction of carbonic acid left would of course become increased after each analysis if allowed to remain in the instrument. It may therefore be advisable to run the air in the left-hand bell-jar through the absorption-tube before commencing an analysis. After the air has circulated three times through the absorption-tube the steadiness of the reading for the volume of carbonic acid absorbed, should the air be passed again and again through the absorption-tube, is usually remarkable, unless there should be an alteration in the temperature of the apparatus, when the reading is observed to follow the changes of temperature. The volume of air introduced into the apparatus to fill up the vacuum is now read off, as also the temperatures of the two bell-jars and absorption-tube. All this is done without drawing the shutters of the case containing the bell-jars, two little windows admitting the hands to work the taps; the absorption apparatus is outside the case at the back (although shown in the plate as inside the case). An instrument which has necessitated such a long description will be thought slow and tedious in its working, but such is far from being the case, considering that the actual analysis will take from 20 minutes to half-an-hour. I expect, however, an increase of speed by substituting a mechanical movement for the hand to turn the handle. Time can probably also be saved by passing the air to be analysed through a tube immersed in water at the temperatre of the bell-jars. When the temperatures and pressures become rapidly steady, the time required for the analysis is of course shortened.

I have not yet found out the shortest period required for depressing or raising the bell-jars; if the movement is too rapid a displacement takes place in the level of the oil in the tanks, and a slight suction follows which of course will be mistaken for so much carbonic acid, a slow movement given to the bell-jars overcomes that objection completely.

No additional time is taken up for running the alkaline solution through the absorption-tube, as this is done by another person while the observer is filling and adjusting. Of course it is only after acquiring some experience that the above speed is obtained.

Compared with Pettenkofer's method, the present instrument claims much greater speed, for while by the former process the solution holding the barium carbonate has to be left a day or over night for the

subsidence of the precipitate, with my instrument the analysis can be made at once, and in a short time. It might be objected that Pettenkofer's method could be carried out as far as the combination of the carbonic acid with the barium, the fluid being preserved for analysis in well-stoppered bottles. A number of determinations in succession might thus be obtained partially made, the final analysis being put off to a future period. I had to adopt this very plan myself in my experiments on the influence of altitude on the chemical changes of respiration, but found it inconvenient in many ways and sometimes objectionable. I am now alluding to air containing over 1 per cent. of carbonic acid; so far my experience does not extend to atmospheric air. I hope soon, however, to be able to determine carbonic acid in the atmosphere by the same means, as I have had a much larger instrument made for that purpose, each bell-jar holding 3 cubic feet of air.

In order to test the accuracy of my instrument, it was necessary to compare analyses of the same air by this means and by another reliable method, and for that purpose I selected that of Pettenkofer which was carried out with one of the cylinders I have formerly described in the "Journal of the Chemical Society," 1880.

The following are eighteen of the comparative analyses made by the two methods:—

Carbonic Acid in 100 *parts of Air analysed.*

First Analysis.

1st—Volumetric method	4·694
2nd „ „	4·633
Mean	4·663
By Pettenkofer's method	4·642
Difference	0·021=0·45 per cent.

Second Analysis.

Volumetric method	4·560
By Pettenkofer's method	4·580
Difference	0·020=0·4 per cent.

Third Analysis.

Volumetric method	4·495
By Pettenkofer's method	4·435
Difference	1·060=1·3 per cent.

Fourth Analysis.

1st—Volumetric method	3·630
2nd „ „	3·670
Mean	3·650
By Pettenkofer's method	3·613
Difference.....	0·037=1·0 per cent.

Fifth Analysis.

1st—Volumetric method	4·478
2nd „ „	4·486
Mean	4·482
By Pettenkofer's method	4·516
Difference.....	0·034=0·75 per cent.

Sixth Analysis.

1st—Volumetric method	4·883
2nd „ „	4·930
Mean	4·906
By Pettenkofer's method	4·891
Difference.....	0·015=0·3 per cent.

Seventh Analysis.

1st—Volumetric method	3·230
2nd „ „	3·210
3rd „ „	3·217
Mean	3·219
By Pettenkofer's method	3·209
Difference.....	0·010=0·3 per cent.

Eighth Analysis.

Volumetric method.....	3·266
By Pettenkofer's method	3·271
Difference.....	0·005=0·15 per cent.

Ninth Analysis.

		Difference from means.
1st—Volumetric method	2·780	0·4 per cent.
2nd „ „	2·768	0·08 „
3rd „ „	2·762	0·3 „
Mean	2·770	
By Pettenkofer's method	2·774	
Difference	0·004=0·15 per cent.	

Tenth Analysis.

		Difference from means.
1st—Volumetric method	4·093	0·07 per cent.
2nd „ „	4·089	0·05 „
3rd „ „	4·088	0·05 „
Mean	4·090	
By Pettenkofer's method	4·044	
Difference	0·046=0·9 per cent.	

Eleventh Analysis.

		Difference from means.
1st—Volumetric method	3·563	0·4 per cent.
2nd „ „	3·560	0·4 „
Mean	3·561	
By Pettenkofer's method	3·575	
Difference.....	0·014=0·4 per cent.	

Twelfth Analysis.

Volumetric method	4·339
By Pettenkofer's method	4·305
Difference	0·034=0·8 per cent.

Thirteenth Analysis.

1st—Volumetric method	2·026
2nd „ „	2·022
Mean	2·024
By Pettenkofer's method.....	2·024
Difference.....	0·000=0·0 per cent.

Fourteenth Analysis.

Volumetric method.....	2·516
By Pettenkofer's method	2·508
Difference	0·008=0·3 per cent.

Fifteenth Analysis.

1st—Volumetric method	2·808
2nd „ „	2·783
Mean.....	2·795
By Pettenkofer's method.....	2·788
Difference.....	0·007=0·25 per cent.

Sixteenth Analysis.

1st—Volumetric method	2·935
2nd „ „	2·939
Mean.....	2·937
By Pettenkofer's method.....	2·939
Difference.....	0·002=0·077 per cent.

Seventeenth Analysis.

Volumetric method.....	4·392
By Pettenkofer's method	4·390
Difference.....	0·002=0·04 per cent.

Eighteenth Analysis.

1st—Volumetric method	0·9327
2nd „ „	0·9412
Mean	0·9369
By Pettenkofer's method	0·9327
Difference	0·0042=0·43 per cent.

These eighteen series of analyses illustrate the results to be obtained by the use of my instrument.

The temperatures of the bell-jars are easily read to a fraction of the tenth of a degree Fahr., and the three corrections for temperature take no time to make. By waiting till the temperatures in the bell-jars,

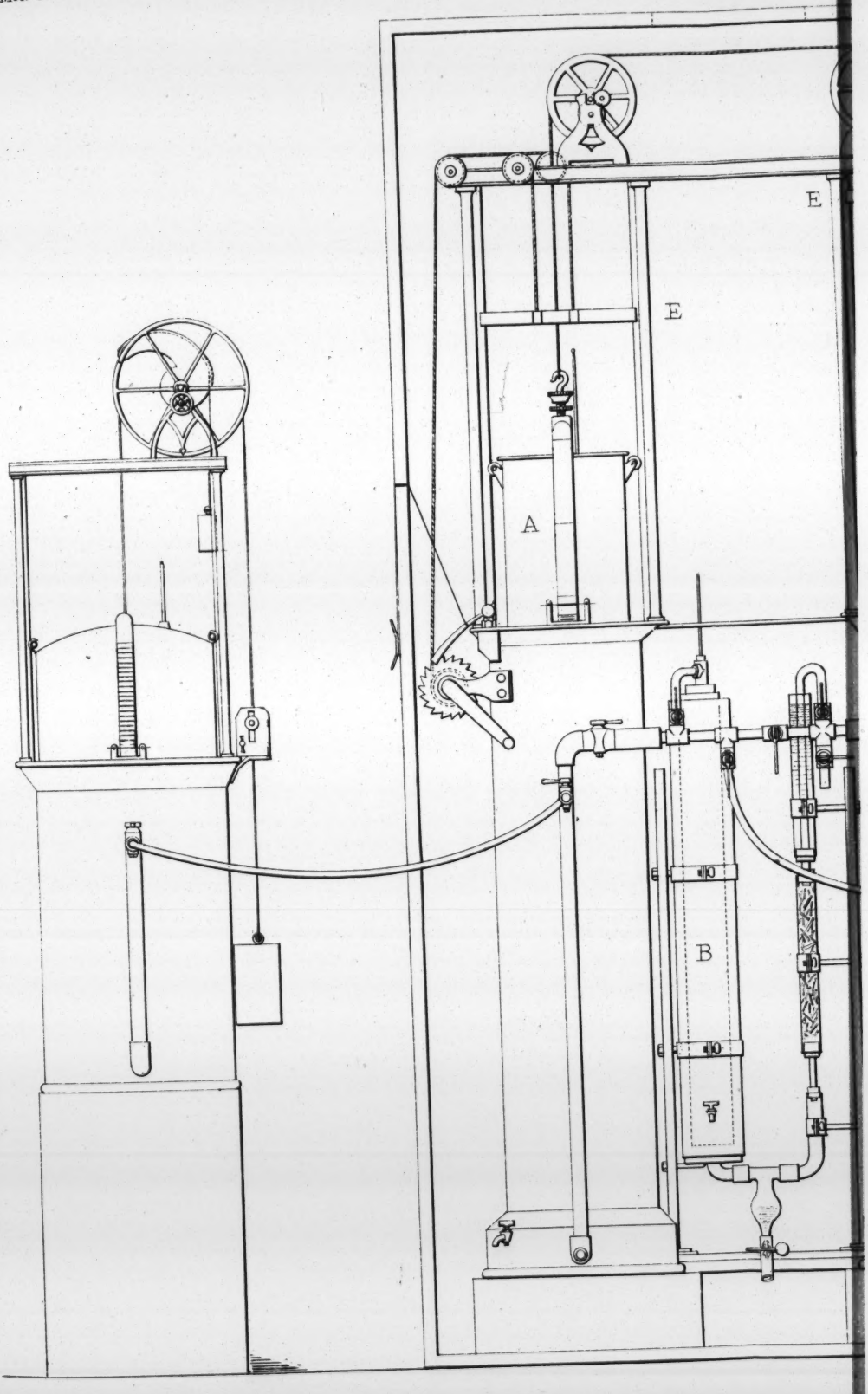
after the absorption of carbonic acid, have risen to their original readings, or very nearly so, the correction to be applied is all but limited to the change of temperature in the 1600 c.c. of the absorption-tube, and is hardly likely to exceed 4 or 5 c.c.

The petroleum gauge is most interesting to watch; when the absorption is complete, after the air has circulated three times through the absorption-tube, the reading of the graduated bell-jar (absolutely under atmospheric pressure) is extremely steady. It remains so for a minute or two, during which time the thermometers rise in both bell-jars; this is owing to the circumstance that the glycerine in the tanks is nearly always slightly colder than the water in the jacket of the bell-jars, and the air on its way through the stand-pipe immersed in the glycerine becomes a trifle colder. As the temperature in the bell-jars approaches its original reading, the pressure from the expansion of the air they contain begins to assert itself, and in order to keep the air under atmospheric pressure the graduated bell-jar must be gently raised. When the thermometers have reached their original height in the jacketed bell-jars of the instrument, the atmospheric pressure again remains exceedingly steady, and there is no difficulty in reading the result. It sometimes happens that after a time, especially in hot weather, the temperature slowly rises by two or three tenths of a degree or more beyond its original reading. In such a case the working of the instrument is so perfect that by introducing the correction required for this increased temperature in the proportion of 1 : 492 of the volume of air analysed per 1° F., the result obtained is the same as it was on the occasion of the first reading.

It is important to consider how far the instrument is reliable. At first I had much difficulty in obtaining in every analysis the same results by the new volumetric and Pettenkofer's methods; at last I found that the only cause of error left was the displacement of the surface of the oil, occasioned by working the bell-jars too fast; when moved slowly, the results by both methods invariably agreed. I conclude that air containing about 4 per cent. of carbonic acid will yield within 0·5 per cent. of the proportion which would be obtained by Pettenkofer's method, and air containing 1 per cent. of carbonic acid will give a result within 1 per cent. of that to be obtained by Pettenkofer's. In the last analysis I have quoted, air containing less than 1 per cent., or 0·9327 per cent. only of carbonic acid, gave with the two methods a difference of 0·45 per cent., a very satisfactory result.

In conclusion I must beg to return my best thanks to Professor Schäfer for having kindly allowed me the exclusive use of a room in the Physiological Department of University College where these researches were carried out; to Mr. W. Parkinson, of the firm of Messrs. William Parkinson and Co., gas engineers, who constructed

Marcet.

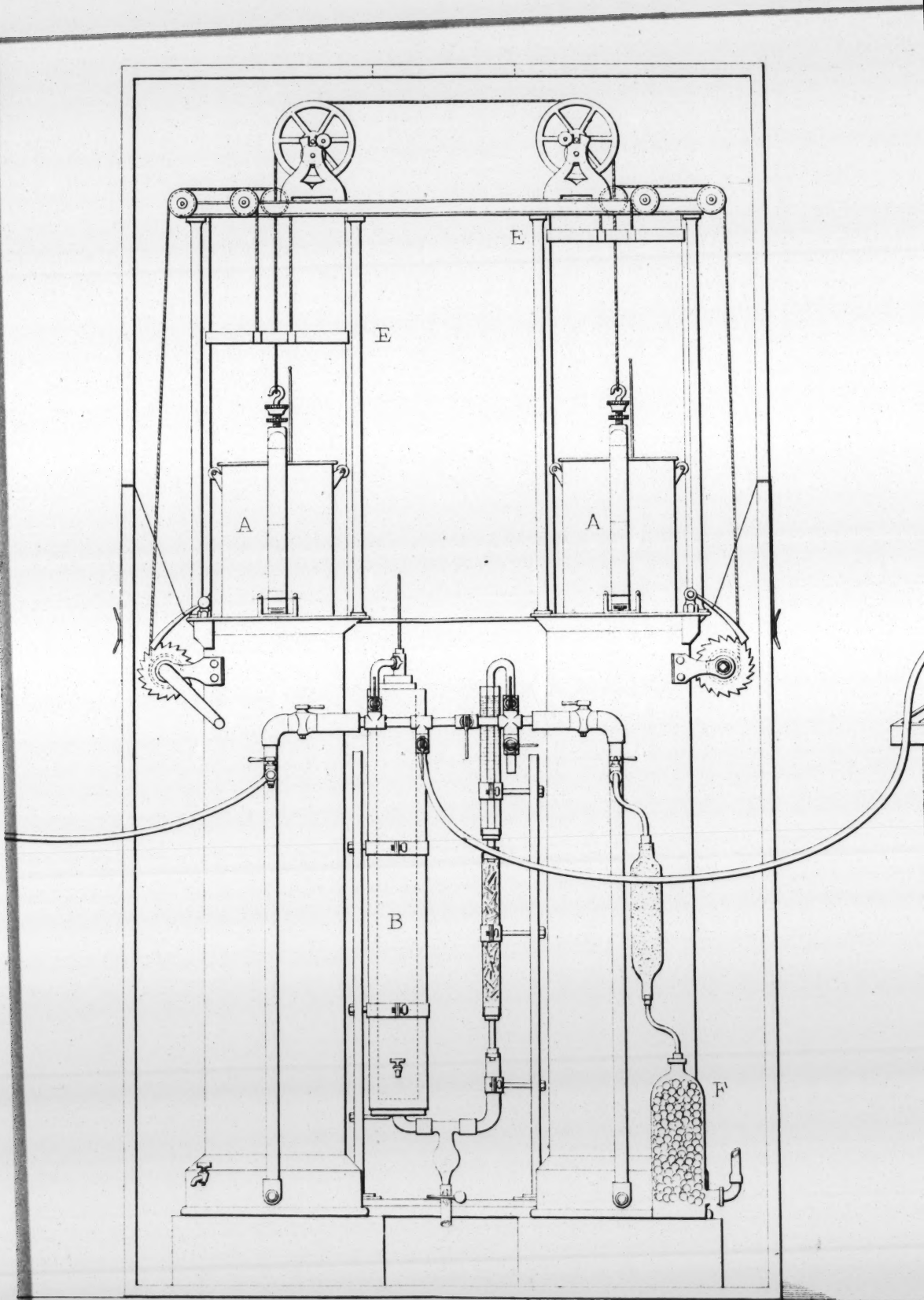


A. A. *Jacketed Air Holders.*

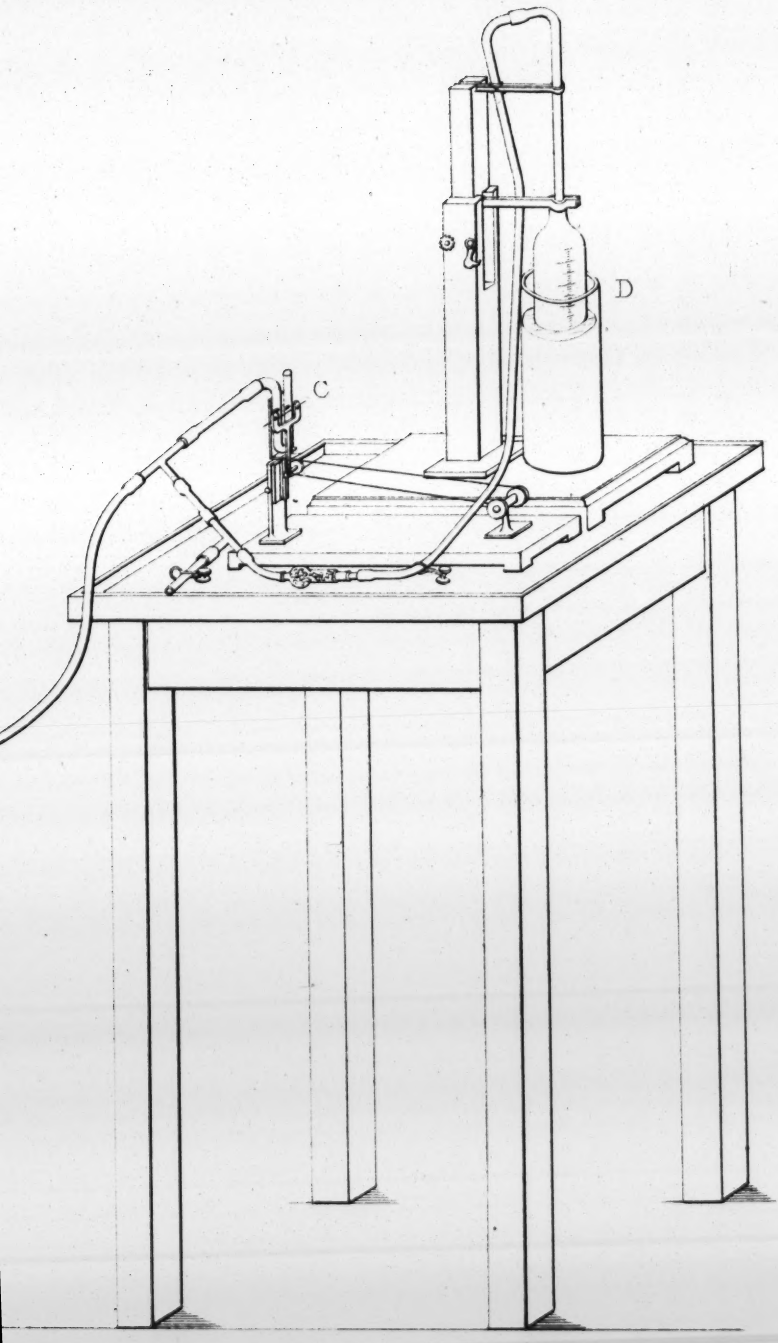
B. *Absorption Apparatus.*

C. *Gauge.*

D. *Graduated R*



A. *Jacketed Air Holders*
B. *Absorption Apparatus.*
C. *Gauge.*
D. *Graduated Receiver.*
E. E. *Iron Weights*
F. *Marbles moist*



West, Newman & Co Imp.

Weights.

es moistened with Potassium Hydrate.

the whole of these instruments for me, and to whom I am indebted for some valuable suggestions; and finally to Mr. Landriset, of Geneva, my assistant since last January, whose care and perseverance, especially with reference to the determinations by Pettenkofer's method, have added not a little to the success of the inquiry.

The drawing of the instrument which accompanies this paper (Plate 2) needs no explanation beyond the few notes referring to letters at the foot of the plate.

"Researches in Stellar Photography. 1. In its relation to the Photometry of the Stars; 2. Its applicability to Astronomical Measurements of great Precision." By C. PRITCHARD, Savilian Professor of Astronomy in Oxford. Received May 20. Read May 27, 1886.

Several attempts have already been made to connect the photographic images of stars with their photometric magnitudes, and consequently with their relative brightness; but hitherto, so far as I know, this relation has been sought by comparing the *impressions* made on the eye rather than as resulting from rigorous measures. With a view to the removal of this indefiniteness, unscientific unless it be unavoidable, I have undertaken a series of instrumental measures of the diameters of the photographic images impressed on sensitised films, which has led to the establishment of a remarkable physical relation (mathematically expressed) between the diameters of the stellar images and their photometric magnitudes, as determined by instrumental means: a method which seems to me to be free from systematic error and personal bias.

With this end chiefly in view, though accompanied also with the hope of obtaining still further, and perhaps even more valuable application of the photographic method to astronomical observations, I procured a number of gelatine dry plates, each being about 2 inches square. The comparative smallness of these plates was determined or suggested by my desire to obtain pictures of such small parts only of the sky as would fall within the ascertained limits of astronomical accuracy of the telescopic field of view, *i.e.*, a field possessing no measurable distortion, and consequently restricted to about a square degree. These plates were exposed in the focus of the well-known De La Rue reflecting telescope, of 13 inches aperture, erected in the University Observatory, at Oxford.

Several plates of the Pleiades were taken with varying times of exposure, and on these were impressed images of portions of the group, extending to stars of approximately the tenth magnitude. The

first use made of these photographic plates was to measure the diameter of those stars whose photometric magnitudes have already been ascertained by the Wedge photometer, and published in the "Uranometria Nova Oxoniensis," p. 94.

Before proceeding further, it is necessary to observe that the photographic images of the stars formed on the film possess in general a very sensible size, even when exposed for moderate intervals of time. It is not necessary here to enter upon the probable cause or causes of these considerable images, or of their peculiar nature; it is sufficient to indicate, as an example, that the image of Alcyone (mag. 3·12) on a plate exposed for (say) ten minutes, is a disk having a diameter of about 0·02 inch, equivalent to a diameter of 30″ in the focal plane of the telescope. As to the peculiar form of these images, it was long ago shown by Bond (in 1858)*, that when viewed under considerable magnifying power, they are not simply sharply defined circular disks, but are fringed all round with discrete black dots, separable under the higher powers of a microscope, and extending to some distance beyond the main black circular disk; nevertheless, when viewed in the comparatively low power of the double-image micrometer, or of the microscope attached to the macro-micrometer, actually used in the measurements, these discrete fringes are not salient, and do not interfere sensibly with the exactitude of the repeated measures.

As a matter of precaution, the diameters of the photographed images were measured, both by means of Airy's double-image micrometer (Greenwich observations), and by the De La Rue macro-micrometer described in vol. 47, "Mem. Roy. Astron. Soc.," and the trustworthiness of these measures was still further confirmed by observations made by means of a stage micrometer applied to an achromatic microscope of very considerable power. The results of these combined measures were then graphically plotted on paper prepared with ruled squares, the measured diameters being taken as the abscissæ, and the corresponding photometric magnitudes taken as ordinates. As usual, a curve was drawn by hand among the points plotted, and thus graphically connecting magnitudes in general with their corresponding diameters. A mere inspection at once suggested the mathematical connexion between these abscissæ and their ordinates, indicating a logarithmic curve, of which the equation would be—

$$y = A\epsilon^{-\frac{x}{a}},$$

where x is the measured diameter, and y is the corresponding magnitude of the star, here expressed on the scale that 2·05 shall represent the magnitude of Polaris, and 2·512 the light ratio in passing from one magnitude to the next. All this is in accordance with the ordi-

* "Astr. Nachr.," vol. 48 (1858), col. 1.

nary convention explained in "Mem. Roy. Astron. Soc.," vol. 47. But it should from the first be understood that any other convenient light ratio, and any other standard magnitude, might have been adopted. It may properly be here repeated that the object of the research is confined to the question whether a definite relation can be discovered between measured photographic images and the ordinary conventional magnitudes of stars.

If this equation expresses the true relation between diameter of image and magnitude, then an equation of condition for the determination of the numerical values of the constants becomes of the form—

$$\log y = \log \mu - \frac{x}{\delta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

the logarithms here being on the ordinary base 10; *i.e.*, if D, D′ be the measured diameters of two stars *on the same plate*, and M, M′ their corresponding magnitudes, then—

$$D - D' = \delta \log \frac{M'}{M} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

From this it follows that in the equation of condition (1), (derived from the original equation to the curve, viz., $y = A\epsilon^{-\frac{x}{a}}$), the constant μ is the magnitude of the faintest star at all impressible on the plate for the exposure in question (or whose photographic diameter is zero), and δ denotes the diameter of that star whose magnitude is the tenth part of that of this zero star on that plate, this particular number (10) being necessarily introduced by the adoption of the Briggean system of logarithms.

With regard to the relation (2)

$$D - D' = \delta\{\log M' - \log M\},$$

it is noticeable that it is in analogy with the fundamental relation existing between light and magnitude—

$$M - M' = K\{\log L' - \log L\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

We now proceed to the numerical computation of the constants (μ) and (δ), and consequently to the comparison of the resulting computed magnitudes with those instrumentally obtained by the Wedge Photometer.

The results of the present research are confined to the discussion of three plates of a portion of the Pleiades, exposed for 7, 12, and 15 minutes respectively on February 11 and April 2 of the present year, at the respective altitudes of 51° 57′, 23° 49′, and 21° 50′. The stars selected for measurement on these plates are twenty-four in

number, viz., those given in the following tables, and of which the magnitudes are given in the "Uranometria Nova Oxoniensis."

Fig. 1.



The curve having been formed as described, and practically represented above, then for each *integral* magnitude, such as D_1M_1, D_2M_2, the corresponding diameter OD_1 OD_2 was read off the curve itself, and the following equations of condition resulted.

$$\log 3\ [0{\cdot}47712] = \mu - 30{\cdot}00\delta$$
$$,,\ 4\ [0{\cdot}60206] = \mu - 23{\cdot}49\delta$$
$$,,\ 5\ [0{\cdot}69897] = \mu - 18{\cdot}69\delta$$
$$,,\ 6\ [0{\cdot}77815] = \mu - 14{\cdot}88\delta$$
$$,,\ 7\ [0{\cdot}84510] = \mu - 11{\cdot}58\delta$$
$$,,\ 8\ [0{\cdot}90309] = \mu - 9{\cdot}00\delta$$
$$,,\ 9\ [0{\cdot}95424] = \mu - 7{\cdot}38\delta$$

A solution of these equations reduces the fundamental equation (1) to—

$$\log y = \log 12{\cdot}82 - 0{\cdot}02157x \quad . \quad . \quad . \quad . \quad . \quad (A)$$

The other two plates, similarly discussed, but each being absolutely independent in its results from the others, give—

$$\log y = \log 12{\cdot}63 - 0{\cdot}0216x \quad . \quad . \quad . \quad . \quad . \quad . \quad (B)$$
$$,,\ y = \log 11{\cdot}69 - 0{\cdot}0247x \quad . \quad . \quad . \quad . \quad . \quad . \quad (C)$$

From this investigation it may be gathered that the magnitude of the star which was commencing to form its photographic image on the

plate (A) after seven minutes' exposure, was 12·82 mag., and that the diameter of a star of the magnitude 1·28, would occupy on the plate in question a circular disk 46·36″ in diameter, or, linearly expressed, 0·024 inch.

Similarly for plates B and C, with the exception that the exposures and the altitudes of the stars are different, to say nothing of other possible differences, such as the chemical constitution of the films, the development of the images, and the meteorological circumstances attending the exposures. It is observable that in Plate B, although the exposure was twelve minutes instead of the seven for Plate A, nevertheless the zero star was *brighter* than for A, owing probably to the lower altitude or to the other causes mentioned above. For Plate C the exposure was still longer, viz., fifteen minutes, nevertheless, as the group was still lower in altitude, the zero star is the brightest of any of the three plates; the instructive character of these phenomena is obvious.

The results of applying the normal equations A, B, C severally to the twenty-four stars whose diameters were measured, are given in the subjoined Tables I, II, III, where, in the first column, is given the star's designation as originally applied by Bessel. In the second is given the measured diameter of its photographed image on the plate in question. This diameter expressed in seconds of arc corresponds to the angular magnitude under which that image would be seen when placed in the focus of the mirror (10 feet), and viewed from the centre of that mirror. In the third column of each table is given the photographic magnitude, computed from the normal equations A, B, C severally. Column 4 contains the photometric magnitude, as given in the "Uranometria Nova Oxoniensis," p. 94. The last column contains the difference between the two magnitudes.

Table I.—February 7th, 1886.

Plate A. Exposure 7 m. Altitude 51° 57′.

Star's designation.	Measured diameter.	Computed (photographic) magnitude.	Photometric magnitude.	Difference C − O in mag.
	″			
η Tauri.....	29·16	3·01	3·12	−0·11
Maia.....	23·21	4·05	3·96	+0·09
Atlas	23·65	3·96	4·00	−0·04
Pleione.....	19·41	4·89	5·46	−0·57
24*p*	15·22	6·02	6·04	−0·02
32.....	13·19	6·66	6·34	+0·32
Asterope (*l*)	14·17	6·34	6·46	−0·12
26*s*	13·34	6·61	6·56	+0·05
24.....	13·21	6·65	6·53	+0·12
29.....	12·80	6·79	6·58	+0·21
12.....	13·13	6·68	6·74	−0·06
19.....	12·24	6·98	6·78	+0·20
22.....	13·89	6·43	6·80	−0·37
31.....	11·34	7·30	6·81	+0·49
10.....	11·87	7·11	7·18	−0·07
8.....	10·74	7·52	7·36	+0·16
20.....	10·63	7·56	7·52	+0·04
21.....	9·82	7·87	7·60	+0·27
18.....	9·62	7·95	7·61	+0·34
9.....	9·93	7·83	7·68	+0·15
30.....	9·41	8·04	7·89	+0·15
15.....	8·49	8·41	8·09	+0·32
13.....	9·09	8·16	8·22	−0·06
36.....	7·90	8·66	9·07	−0·41
35.....	6·14	9·45	9·67	−0·22

Table II.—April 2nd, 1886.

Plate B. Exposure 12 m. Altitude 21° 50′.

Star's designation.	Measured diameter.	Computed (photographic) magnitude.	Photometric magnitude.	Difference C − O in mag.
	″			
η.....	28·74	3·03	3·12	−0·09
Maia.....	23·47	3·93	3·96	−0·03
Atlas	26·68	3·71	4·00	−0·29
Merope	21·41	4·46	4·30	+0·16
Pleione	19·70	4·75	5·46	−0·71
24*p*.....	16·00	5·71	6·04	−0·33
32.....	13·53	6·45	6·34	+0·11
26*s*.....	13·80	6·37	6·56	−0·19
24.....	13·84	6·36	6·53	−0·17
29.....	12·70	6·72	6·58	+0·14
12.....	12·29	6·86	6·74	+0·12
19.....	12·67	6·73	6·78	−0·05
22.....	13·55	6·44	6·80	−0·36
31.....	11·60	7·10	6·81	+0·29
17.....	12·28	6·86	6·84	+0·02
33.....	12·55	6·77	6·78	−0·01
10.....	11·26	7·22	7·18	+0·04
8.....	10·79	7·39	7·36	+0·03
20.....	10·46	7·51	7·52	−0·01
18.....	10·00	7·69	7·61	+0·08
7.....	10·53	7·49	7·54	−0·05
9.....	9·90	7·73	7·68	+0·05
30.....	9·76	7·78	7·89	−0·11
15.....	8·56	8·26	8·09	+0·17
13.....	9·14	8·02	8·22	−0·20

Table III.—April 2nd, 1886.

Plate C. Exposure 15 m. Altitude 23° 49′.

Star's designation.	Measured diameter.	Computed (photographic) magnitude.	Photometric magnitude.	Difference C − O in mag.
η	23″·04	3·16	3·12	+0·04
Maia	17·85	4·33	3·96	+0·37
Atlas	19·51	3·85	4·00	−0·15
Merope	16·94	4·46	4·30	+0·16
Pleione	16·36	4·61	5·46	−0·85
24p	11·85	5·96	6·04	−0·08
32	10·54	6·42	6·34	+0·08
26s	11·23	6·27	6·56	−0·29
24	10·25	6·53	6·53	0·00
29	10·09	6·59	6·58	+0·01
12	10·23	6·55	6·74	−0·19
19	9·60	6·78	6·78	0·00
22	10·40	6·47	6·80	−0·33
31	8·59	7·17	6·81	+0·36
17	8·66	7·14	6·84	+0·30
33	8·84	7·07	6·78	+0·29
10	7·69	7·55	7·18	+0·37
8	8·21	7·33	7·36	−0·03
20	8·18	7·35	7·52	−0·17
18	7·40	7·67	7·61	+0·06
7	8·03	7·40	7·54	−0·14
9	7·18	7·77	7·68	+0·09
30	6·29	8·17	7·89	+0·28
15	5·92	8·35	8·09	+0·26
13	6·49	8·08	8·22	−0·14

A comparison of the numbers in the last column affords a definite indication that there does exist in nature a general definite relation between the photographic diameters and the intensities of the stellar lights impressed on the plate, and that this relation is truly represented by the equation—

$$D - D' = \delta\{\log M' - \log M\}.$$

Nevertheless it is also made evident that although this relation is a fact for stars *on the average*, there are a few stars which stand out from that average, a result which might have been anticipated on account of the salient character of special spectra occasionally met with in certain stars. In the group examined above, the stars having this abnormal actinic action are Pleione, and Nos. 22 and 31. These stars give the same evidence of peculiarity in all the three plates; and from this circumstance it becomes clear that if a comparison is at any time to be made between the photographic and the photometric magnitudes, more than one plate should be taken, in order to be sure that any discordances are due to variations in the actual actinic action, and not to accidental circumstances. Omitting these three stars, the average deviation of the magnitude here computed photographically from that photometrically determined, is 0·16 mag. If these three stars be included it is 0·19 mag. Had it not been that the Pleiades group was rapidly approaching the sun at the time of these observations, the character of the spectra of the three exceptional stars would have been examined, with the view of explaining, if possible, their peculiar actinic action on the photographic film.

The general results of the foregoing research appears to be—

First. There does *in general* exist a remarkable and definite relation between the intensity of the actinism of a star on a photographic film, as expressed by the area of the image formed, and the intensity of the light as measured by a photometer. That relation also exhibits a certain analogy to the relation between the photometric magnitude and the actual intensity of the light of a star. These relations being expressed by the formula—

$$\frac{M}{M'} = K \log \frac{L'}{L}$$

$$\frac{D}{D'} = K' \log \frac{M'}{M}$$

where the relation is sufficiently evident.

Secondly. *Both* these relations are disturbed in the cases of any salient difference in the colours of particular stars, or at all events present analogous difficulties in dealing with them by way of definite measurement.

Thirdly. If it be established that the photographic film is permanent, we possess in the application of photography the certain and definite means of comparing at pleasure any changes which in the lapse of time (whether longer or shorter) may occur in the lustre of any star or group of stars, without the labour of continually measuring and recording the actual magnitude at any particular epoch. We possess, that is, a register, which can be examined when the emergency arising from any suspicious circumstance occurs.

II.—*On the Relation of the Dimensions of the Photographed Images of a Star to the Time of the Exposure of the Plate.*

It will be observed that so far as the foregoing investigations are concerned, the stars examined and compared are all on the same plate and exposed for the same time; and it is from such plates that the remarkable relation between the areas of the images and the photometric magnitude has been obtained. There is, however a still wider question than this, involving the alterations in the disk-areas caused by the variations of the time of exposure of any particular plate on which they are impressed. I do not propose to enter upon this question to the exhaustive extent which at the present day is required, by the extreme and hitherto unexpected duration of the times of exposure, extending now to hours rather than to minutes or seconds. I will here only explain that the question of the relation of time of exposure to the photographic diameter of a particular star impressed on a film was elaborately examined by Bond.* From exposures varying from one second to two minutes, he came to the conclusion that the area of a given star's photographic image impressed on a given plate varied directly as the time of exposure. The plates he used were the ordinary wet collodion plates of 1858. My own investigations, based on exposures even when limited to the same time as Bond's (but on the modern gelatine dry plates), led me to a very different conclusion, viz., that the area varies as the square root of the time of exposure.

Before, however, publishing the details of my observations, it is desirable to extend the time of exposure to far larger durations, and I abstain at present from further remarks thereon, with the exception that, judging from the remarkable photographs taken by the Brothers Henry at the Paris Observatory, Bond's conclusions appear to me (as at present advised, but subject to further examination) incompatible with observed fact. There are also some still more remarkable and interesting investigations relating indirectly to the same question, made recently at Potsdam by Dr. Lohse ("Astr. Nachr.," vol. 111 (1885), col. 147), which may require remark, and I hope at an early

* "Astr. Nachr.," vol. 49 (1858), col. 81.

opportunity to make the whole subject the basis of a further communication. I will, however, add here that I have found the fringes of discrete dots which surround the images on the original negative entirely disappear on transferring them to a positive print, and leave their positive images simply as clean cut disks, whose diameters are sensibly the same as the measured diameters of the concrete disks on the original negative. It is conceivable that in printing, the light penetrates through the interstices of the fringe.

III.—*Applicability of Photography to Astronomical Measurements of Great Precision.*

The scientific interest and importance attached to the possession of a register of the relative lustre of the stars, virtually exhibited on a permanent photographic film, capable of re-examination at any time, and accessible at will, are, if possible, surpassed by the value attachable to the measurability of their relative positions, provided only the accuracy of the measures made upon the film is equal to that obtainable by the application of the best astronomical instruments to the sky itself. Three questions, in fact, here present themselves in relation to this matter. The first is, whether the photographic pictures accurately represent a portion of the skies; the second is, whether the relative co-ordinates of the star images are measurable with as much delicacy and certainty as they are by the heliometer, or any other form of micrometer, applied to the sky itself: and the third question has reference to the permanence of the photographic film.

For present convenience, I shall consider the reply to the first two of these questions as involved in the same evidence. In order to settle these points, several photographic plates of stars situated in portions of the Pleiades group, already referred to, were selected for examination. On each of these the image of η Tauri (Alcyone) is impressed, together with that of many other stars in the group, and specially for this purpose twenty-five of the stars, whose distances from Alcyone had been so carefully measured by Bessel with his famous heliometer in 1838—1842. These distances from Alcyone on the several films were each measured, and as far as possible with as many repetitions as those adopted by Bessel himself. Inasmuch as the plates were taken at different altitudes on different nights, the necessity arose for applying small numerical corrections for the varying effects of refraction, and where necessary, other small corrections for aberration were applied, in order that the resulting distances might be strictly comparable *inter se*. This examination of the amount of correspondence in the measures is the only object here, for the moment, entertained.

As far as the general mass, or great majority, of these observations is concerned, it is not necessary to give all the particulars, but I have selected the first sets of measures as affording an average specimen of the whole work. These results are exhibited in Table IV; and in Table V are given the mean deviations of the whole twenty-five stars, deduced as in the case of the six for which the complete details are printed in Table IV.

Table IV.—Comparison of Relative Accordances of the Photographic Measures of Distances of Stars from Alcyone *inter se*, with similar measures made by Bessel.

Number of plate.	Distance measured		Deviation from the mean	
	On photograph.	With heliometer.	On photograph.	In heliometer.
Star 7.				
Plate I.....	1355″·65	1355″·02	0″·27	0″·04
„ II.....	1355·11	1355·39	0·27	0·41
„ III.....	1355·43	1354·74	0·05	0·24
„ IV.....	1355·32	1354·76	0·06	0·22
Mean	1355·38	1354·98	0·16	0·23
Star 8.				
Plate I.....	1081″·39	1080″·27	0″·51	0″·12
„ II.....	1080·68	1080·48	0·20	0·33
„ III.....	1080·79	1080·40	0·09	0·25
„ IV.....	1080·85	1080·25	0·03	0·10
„ V.....	1081·04	1079·62	0·16	0·53
„ VI.....	1080·52	1079·85	0·36	0·30
Mean	1080·88	1080·15	0·22	0·27
Star 9.				
Plate I.....	1045″·12	1045″·07	0″·00	0″·22
„ II.....	1045·21	1045·06	0·09	0·21
„ III.....	1045·00	1044·88	0·12	0·03
„ IV.....	1045·13	1044·39	0·01	0·46
Mean	1045·12	1044·85	0·05	0·23

Table IV—*continued.*

Number of plate.	Distance measured.		Deviation from the mean.	
	On photograph.	With heliometer.	On photograph.	In heliometer.
Star 10.				
Plate I.....	1003″·35	1002″·63	0″·47	0″·13
„ II.....	1003·00	1002·57	0·12	0·07
„ III.....	1002·11	1002·49	0·77	0·01
„ IV.....	1003·06	1002·29	0·18	0·21
Mean	1002·88	1002·50	0·38	0·11
Star 12.				
Plate I.....	1548″·58	1547″·27	0″·65	0″·29
„ II.....	1547·94	1547·47	0·01	0·09
„ III.....	1547·98	1547·86	0·05	0·30
„ IV.....	1547·21	1547·65	0·72	0·09
Mean	1547·93	1547·56	0·36	0·19
Star 13.				
Plate I.....	521″·99	521″·61	0″·14	0″·23
„ II.....	522·21	521·52	0·08	0·14
„ III.....	522·15	521·20	0·02	0·18
„ IV.....	522·15	521·17	0·02	0·21
Mean	522·13	521·38	0·06	0·19

Table V.

Star.	Mean deviation		Star.	Mean deviation	
	On photograph.	In heliometer.		On photograph.	In heliometer.
	″	″		″	″
No. 7	0·16	0·23	No. 24	0·27	0·20
„ 8	0·22	0·27	„ 27	0·18	0·11
„ 9	0·05	0·23	„ 29	0·03	0·25
„ 10	0·38	0·11	„ 30	0·32	0·47
„ 12	0·36	0·19	„ 31	0·48	0·36
„ 13	0·05	0·19	„ 32	0·17	0·37
„ 15	0·31	0·20	„ 33	0·45	0·42
„ 24*p*	0·23	0·26			
„ 17	0·23	0·33	Merope	0·24	*
„ 18	0·12	0·18	Maia	0·31	*
„ 19	0·09	0·71	Atlas	0·13	*
„ 20	0·39	0·43	Pleione	0·28	*
„ 22	0·37	0·43	26*s*	0·24	*

* For these five stars, Bessel has not expressed the individual results in seconds of arc.

The average discordance for the entire group is in the case of photographic measures 0·24″, and in the case of heliometer measures 0·29″.

In the above preceding clause I have adopted the term *general mass*, because on one of the plates (fig. 2) there occurs a somewhat curious and instructive fact, viz., that while for nineteen instances of stars measured from η Tauri, the distances agree with those of the same stars on the other three plates, there is a sensible discordance between the measures of Atlas, Pleione, Nos. 29, 31, 32, and 33, when compared with the similar measures on the three other plates. This fact indicates a distortion of the film at the places where these six stars are impressed. On inspecting the diagram of the plate, it is observable that all these six stars, exhibiting these discordances of distance from η, lie on the same portion of the plate. It follows, necessarily, that three of the plates must accurately represent the portion of the heavens impressed, and the fourth plate in those portions only which exhibit an identity with the other three. From this fact we conclude that it is not safe to regard any single plate as necessarily accurate in itself, unless it is corroborated by the identity of exact measurements, when compared with others also. The discordances here referred to are small, but existent, perceptible, and in themselves destructive of accurate astronomical conclusions involved therein.

FIG. 2.

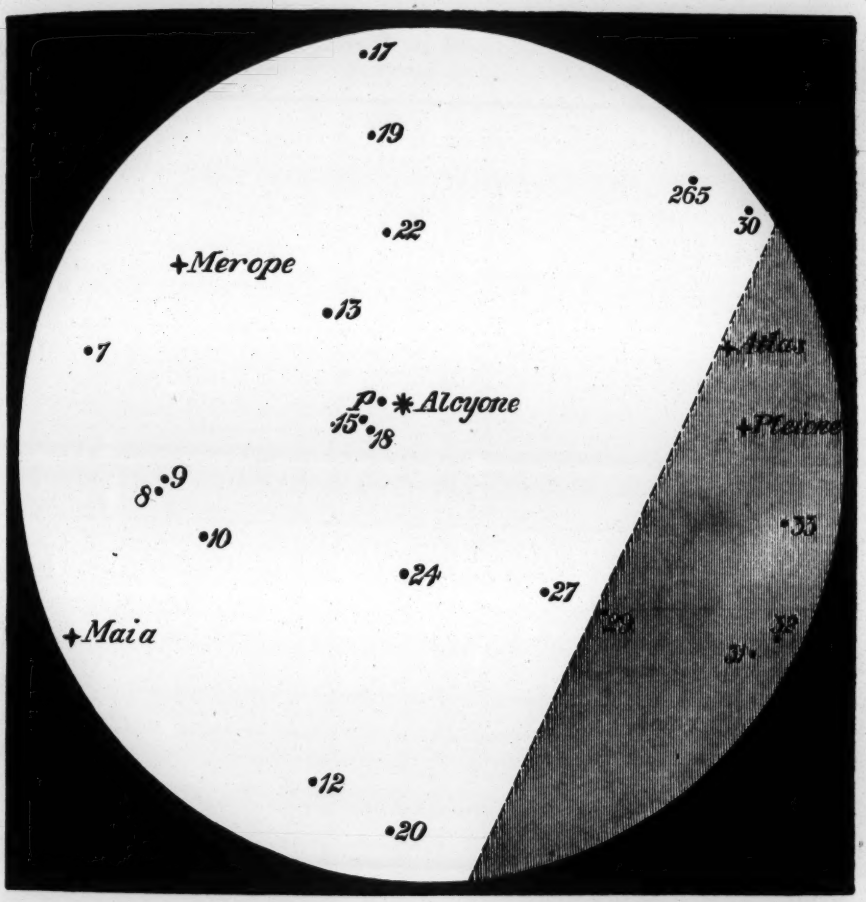


Diagram of stars in the Pleiades, as seen in an inverting telescope. The portion of the film distorted is shown by the shaded portion.

I now proceed to give the details of the individual measures of the six stars which present the anomaly in question, and contiguous to them will be found the accordances of the similar measures on the three other plates.

Table VI.—Discordant Measures of the Six Stars on the Disturbed Film. Plate IV.

Number of plate.	Atlas.	Pleione.	Star 29.
Plate I.....	1391″·14	1402″·67	1202″·23
„ II.....	1391·46	1403·50	1202·29
„ III.....	1391·42	1403·08	1202·31
Mean	1391·34	1403·08	1202·28
„ IV discordant ...	1391·77	1403·95	1203·04
Number of plate.	**Star 31.**	**Star 32.**	**Star 33.**
Plate I.....	1806″·11	1832″·58	1678″·39
„ II.....	1807·39	1832·89	1679·56
„ III.....	1806·99	1833·02	1679·24
Mean	1806·83	1832·83	1679·06
„ IV discordant ...	1808·55	1834·15	1680·34

It may be noticed that the discordances for Atlas and 29 are not so conspicuous as for the remaining stars, indicating that the principal disturbance of the film has taken place in the neighbourhood of 31, 32, and 33. The maximum discordance applies to the Star 31, where it is 1·7″ ; and the least disturbances applies to *Atlas*, where it does not reach half a second of arc.

The accordance of the measures given in Tables IV and V *inter se*, is quite equal to that exhibited by the measures of the illustrious astronomer of Königsberg. I confess that, having already had a long experience of the results of repeated measures of distances of spots on the moon as obtained from lunar photographs ("Mem. Roy. Astron. Soc.," vol. 47 (1883)), and being at the same time aware also of Dr. De La Rue's great success in measuring solar photographic images, I was not altogether surprised, but agreeably satisfied by the removal of all doubt and uncertainty. But, in reference to this question, we must not overlook the fact of the generic distinction which exists between the circumstances attending the production of these solar and lunar photographs and these stellar images formed on photographic films of a totally different description, and which latter films do necessarily present difficulties and suspicions not applicable to the former. In the former case the collodion plates were exposed for at most a very few seconds, or even for fractions of a second; but in the case of the star plates, the exposures last, not only through very many minutes, but the several star images make their appearance, according to their

relative brightness, only after varying times of exposure. It would therefore have been premature, or even unscientific, to argue *à priori* of the reliability of the latter measures from those of the former.

From inspection of Table V it will be found that the general error of the whole group of measures of the photographic plates, regarded as measures of the same quantity, is 0·24″, and of the heliometer 0·29″. Even this minute error would probably have been smaller had it not been for a practical difficulty, which arises in the accurate bisection of the disks of the star images. This difficulty, though it exists also to a proportionate extent in the use of any filar micrometer applied to the brighter stars in the skies, does not exist to an equal degree in the case of heliometer measures. Nevertheless, such is the convenience of leisurely bisecting even the comparatively large photographic disk on a steady film, that the final results of the photographic measures are, on the whole, slightly more accordant than those secured by the use of the heliometer. In point of facility and rapidity, the advantages of the photographic method are enormous.

Having mentioned the error necessarily connected with bisection of the star disk, it is desirable to present a specimen of what actually occurs. Accordingly I give a short table, containing the detail of the errors made in continually bisecting two star disks, such as actually occurred in reference to Maia and Asterope, of diameters 25″ and 15″ respectively.

Table VII.

Maia, mag. 4·00.		Asterope, mag. 5·98.	
Reading of screw at apparent bisection.	Difference of readings from mean in seconds of arc.	Reading of screw at apparent bisection.	Difference of readings from mean in seconds of arc.
in.	″	in.	″
0·90822	0·17	0·90774	0·25
0·90811	0·36	0·90772	0·28
0·90826	0·10	0·90793	0·09
0·90841	0·15	0·90799	0·19
0·90859	0·46	0·90785	0·05
0·90861	0·50	0·90788	0·00
0·90870	0·65	0·90803	0·26
0·90846	0·24	0·90791	0·05
0·90827	0·09	0·90788	0·00
0·90810	0·38	0·90781	0·12
0·90816	0·27	0·90779	0·16
0·90811	0·36	0·90799	0·19
0·90816	0·27		
Mean 0·90832	0·31	0·90788	0·14

Comparing this liability of error in the bisection of each star, in obtaining its distance from η, with the whole liability to error in distance, which is 0·24″, it follows that probably the whole error in the distance measures arises from the bisection, and that therefore the film has remained undisturbed in the direction of distance. Another practically important conclusion is that the exposure of the plates should not continue beyond the time necessary to form distinct disks of the stars to be measured, inasmuch as an increased enlargement of the photographic image necessarily entails an increased error in its bisection (see Table V). Hence, if the stars to be measured differ very greatly in magnitude, then it may be desirable to connect the brightest stars with the fainter by means of stars of intermediate magnitude.

Such, then, appears to be the comparatively rigorous character of photographic measurement: but I hope to carry the investigation still further, by the application thereof to the determination of stellar parallax, for which the method appears to be eminently adapted.

Another investigation presents itself in the inquiry as to the effect of atmospheric absorption at varying altitudes on the image impressed.

With regard to the question of the permanence of the film, it would at present be premature to speak: it might be unsafe to argue from the known permanence of the collodion plates on which the lunar images are impressed, but so far as the present knowledge suggests there is no evidence of deterioration. I propose, however, to measure a few distances periodically, as well as the diameters of the larger star disks.

"Contribution to the Study of Intestinal Rest and Movement."

By J. Theodore Cash, M.D. Communicated by T. Lauder Brunton, M.D., F.R.S., &c. Received May 25. Read May 27, 1886.

During the last two years, as opportunity has permitted, I have been engaged in an experimental study of the various factors which contribute to movement or rest of the small intestine.

This research has furnished me with much information of an analytical character, but it seemed desirable to supplement and, in some degree, to control this, by investigating the general conditions which influence such changes in a healthy animal in which all the functions exerting an influence upon the viscera are in full operation. The most promising means for effecting this purpose appeared to be the establishing of a Vella's fistula which would permit of thorough

examination without the necessity of subsequent employment of anæsthetics or of operative procedure.

This operation consists in making a double division in the continuity of the small intestine, the approximation by means of sutures of the upper and lower ends of the gut which are to be returned to the abdominal cavity to serve their previous purpose as a part of the intestinal tube, the bringing outwards of the two ends of the intestine which has been isolated from the rest of the tract by the double section, with a view to the subsequent healing of these two mouths in the abdominal incision. The mesentery is interfered with as little as possible in making the intestinal division.

Operation.—Upon a healthy dog weighing 14 lbs. deeply under the influence of morphia, I performed this operation, separating from the rest nearly 20 cm. of the upper portion of the jejunum, which was dealt with as above described.

The dog was kept warm, and the day after the operation received several spoonfuls of milk. The quantity was increased rapidly, and on the fifth day meat was given; by the sixth day the animal was practically normal, the condition of the wound in the abdominal walls, which was now granulating and painless, being all that could be desired. A further rest of three days was, however, allowed in order that all danger of prolapse of the intestine in lifting the animal might be avoided.

No hypnotic or anæsthetic was employed in any of the experiments to be described in this paper, nor was any more restraint exercised than by occasionally laying a hand on the head or body of the dog in token that he was to remain still. As regards the disposition of the animal, his attachment to myself and the laboratory attendant was such, and the inconvenience of the experiment so little, that when brought into the laboratory he often endeavoured to leap on to the table on which he rested whilst under examination. When placed on his side on this table he remained, as a rule, perfectly quiet, except for an occasional deep respiration, a start during sleep, or a stretching of the limbs.

On examining the floor of the short wound in the middle line of the abdominal parieties, the two round mouths of the fistulous intestine with their everted mucous membrane could be readily observed, and their movements noted without the least difficulty. There was a space of 2 cm. intervening between these two mouths which was filled by the parietal peritoneum muscles and skin which had been divided in the operation, and subsequently united by suture to the corresponding structures of the opposite side. At the time the experiments commenced firm union had occurred in this position.

Observation of the Fistulous Opening during Hunger, Feeding, Exercise, &c.

If the animal was in a condition of hunger (it received one large meal of meat about the middle of each day, and manifested a desire for food therefore when brought up to the laboratory about 10 o'clock in the morning) the appearance of the mouths of the fistula varied from time to time.

After a certain amount of movement had taken place, it often happened that a period of complete repose of 3 or even of 8 to 12 minute occurred without any contraction making itself manifest; then, spontaneously, movements of one or both mouths of the fistulous gut made their appearance, which sometimes appeared to be either sequences to acts of swallowing on the part of the animal, or results of mental impressions.

These contractions usually evidenced a very regular rhythm, but varied greatly in strength.

Those first occurring were frequently so slight that they could only be observed by the change of position in the light reflected by the moist mouth of the fistula. Increasing in strength they reached a maximum, and, after persisting for a time (30″ to 3′ or 4′), gradually declined or suddenly ceased. When peristalsis followed an act of deglutition, the effect was of shorter duration and culminated more rapidly, duration of the contractions was usually shorter, and the advent of their maximal strength more rapid.

If when only gentle and occasional movement of the mouths of the fistula, attended by little or no extension of secretion, was occurring, the animal was allowed to jump down on the floor and encouraged to run a few times round the room, it was found on continuing the observation after the exercise, that the contractions were much more frequent and powerful, and were sometimes associated with the ejection of increased secretion from the lower fistulous opening.

During the condition of hunger, *i.e.*, before the animal had received its daily meal, the smell of food, but much more markedly the swallowing of several pieces of meat successively presented to the animal, had for effect a regular and powerful series of contractions, usually attended with an increased ejection of secretion.

It was evident therefore that without the least interference with the fistulous intestine, the appearances coincided with the experiments I recorded graphically, and which I shall now consider in more detail.

Graphic Experimental Records.

For the purpose of recording merely an intestinal contraction near the orifices of the fistula, a fine glass cannula slightly enlarged at the end, upon which was fixed a small bag of delicate elastic membrane,

was employed. This constituted the "sound," and was connected by means of a system of thick-walled india-rubber tubes with a mercurial manometer or a Marey's recipient tambour; if the former was employed water was the means of communicating the impulse from the compressed sound; if the latter, air. The sound, whether inflated with air or distended with water, had a diameter when fully filled of 7 mm. at its widest point and a length of 12 mm. The manometer was so arranged that the level of the mercury was as nearly as possible that of the sound resting in the intestine. Occasionally two sounds, each connected with a separate manometer, were used, one being introduced through the upper, the other through the lower mouth of the fistulous intestine. When it was desired to register the speed of propulsion of a solid body through a portion or the whole of the length of the fistulous intestine, a modified form of the method used by Mosso, and developed in its details by Ranvier in experiments upon the œsophagus, was employed. In this case the travelling sounds were oblong bodies having rounded ends, and measuring in breadth from 5 to 9 mm., in length 12 to 14 mm., and were made of metal, cork hollowed out, or glass, and in several experiments a solid sound was replaced by a small piece of lean meat. Those made of metal were the thin-walled silver capsules which Prof. Kronecker employs for the introduction of his maximal thermometers into the alimentary canal of animals. They are perforated with one or two holes which serve for the attachment of a thread of silk. The larger of the metal capsules which I employed as a travelling sound was 9 mm. broad by 14 long, and weighed 3·4 grams; the smaller, 6 mm. by 14, weighed 2·2 grams.

The registering apparatus consisted merely of a thin wedge of cork bearing a glass pen, and travelling vertically by means of two glass eyes passed through its substance upon parallel steel guides. The weight of this falling pen was 2 grams. A fine silk thread from the sound to the pen passed over two pulleys, one placed opposite and in the same plane as the fistulous opening, the second vertically above the steel guides of the travelling pen. When traction was made by the sound the pen was drawn upwards, its elevation being directly proportional to the extent of withdrawal of the sound from the lower pulley.

For each observation the dog was placed on its side with the fistulous opening opposite to and at the same level as the lower pulley; the sound was introduced into the upper fistulous opening, the upper pulley raised so that the pen was drawn upwards for a few centimetres from its support,* and the drum arranged so as to leave the whole

* The respiratory movements were to some extent communicated to the pen connected with the sound. By means of two Marey's tambours the number of respirations was usually written at the same time as the curve of intestinal movement.

breadth of its recording surface available for registration, when the pen should be raised by passage of the sound more deeply into the fistula. A slow rotation of the drum, usually one revolution in about 17′, was employed.

There is no purpose in separating the results according to variation in the experimental method, especially as the fixed sound and the travelling sound were employed with one or two unimportant exceptions in each class of experiment.

It may be stated generally that although during a condition of fasting occasional contractions make themselves manifest, the record obtained from a fixed sound sunk 4 cm. into the fistulous intestine shows that periods of complete quiescence are the rule, and movement (as evidenced by contraction around the sound) the exception. When a period or phase of peristaltic activity begins, the first contraction manifests itself, usually by a faint impulse communicated from the sound of the registering apparatus. This is generally succeeded by a series of waves of uniform frequency, but varying in their extent. The repetition of the contractions may, however, show a slight variation; thus, during the first part of a phase the wave contractions may be at first eight in a minute, then nine, and again eight before cessation.

The following notes fairly indicate the phase of quiescence and of contraction occurring in the intestine.

Experiment Records.

Experiment I.—The sound, slightly distended with water (diameter 5 mm.), was introduced into the upper fistulous opening; the animal had not fed for fourteen hours, and was evidently hungry.

0′. Introduce sound.
5′. Up to this time rest.
5′ to 6′ 40″. Feeble contractions, succeeded by 6′ rest.
13′ to 16′. Gentle contraction succeeding slight movement of animal during which it swallowed.
26′. About a teaspoonful of water was introduced into mouth; as a result three distinct swallowing movements occurred; intestinal contractions developed themselves which continued strongly for 2′; faint contraction for the succeeding 3′.
31·5′ to 39·5′. Rest, except for very faint contractions which may have been communicated by a loop of adjacent intestine. Swallowed, but no definite contraction resulted.
39·5′ to 42·5′. Very faint contractions.
42·5′ to 50·5′. Pause.
50′ 5′ to 53·5′. "Empty" swallow succeeded by feeble contractions for 3′.

53·5′ to 91′. There have been three series of contractions, lasting in all 12′. Two of these were preceded by swallowing, which in the first case was spontaneous, and in the second resulted from rubbing the larynx.

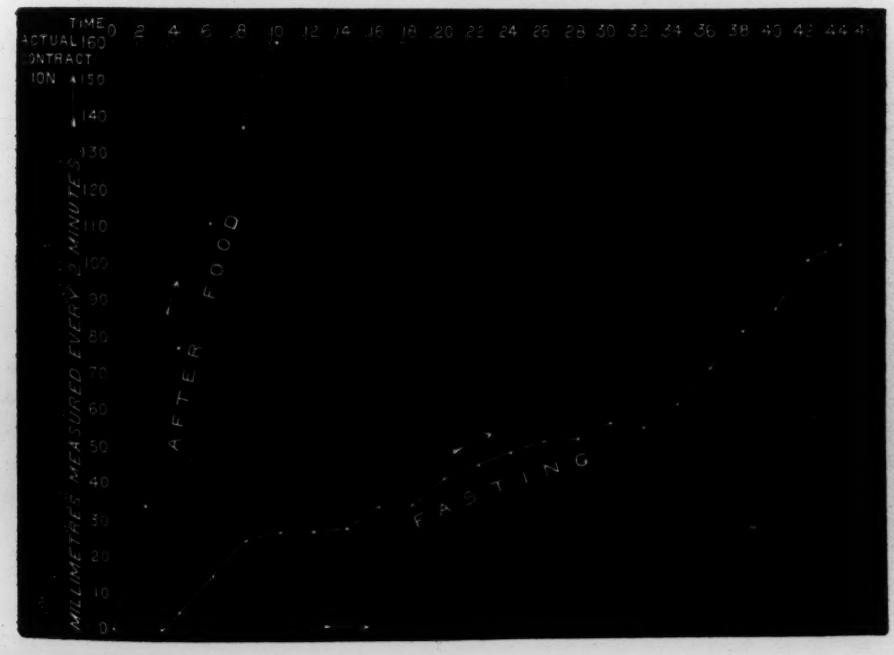
Total in 91′.—In this experiment made upon the fasting animal, out of a total of 91′ active intestinal contraction was present during 9′, faint contractions for 19′ more, and the intestine was at rest for the remainder of the time, *i.e.*, 63′. One-half of the series of contractions were preceded by spontaneous or induced acts of deglutition.

It cannot be demonstrated by the fixed sound method that the contractions are any more than local contractions, but as the occurrence of these for any length of time greater than 1′ or 2′ is very exceptional, and is almost always a prelude to propagated contraction, we may assume with certainty that many of the long-continued phases of contraction were propagative, or in other words they would have propelled the sound downwards if it had been movable.

From simple observation of the fistulous opening made immediately afterwards, no sound having been introduced, I found that the number of the series of contractions were in the two cases equal, but that in the sound experiment their duration was slightly longer.

An experiment made during a condition of hunger, but with a travelling sound, will now be recorded.

Experiment II, Chart I. Experiment VII, Chart VI.



Experiment II (Chart I (continuous line, diagrammatic), and Curve I).—The dog had not been fed for nineteen hours. A small sound consisting of ½ gram of lean meat on which a little piece of tendon was left, was attached by a thread to the pen already described which moved vertically upon guides.

This sound was introduced deeply into the upper fistulous opening. The following notes indicate its progress through the fistula:—

Experiment II, Curve I.



Reduced scale = ⅓th.

0′. Sound introduced into the intestine.
4′. Definite peristalsis has commenced; animal sleeping.
7′. Roused; a little movement with deep respiration.
17′. Again asleep; gentle peristalsis.
19′. Started in sleep and awoke. The loop of intestine was evidently twisted or moved away from former position (roll movement) to which it shortly returned; again sleeping.
21′ 5″. Roused. Deep respiration with slight peristalsis.
33′. Propulsive peristalsis, strong and regular.
Dog roused just before expulsion of sound from the lower opening. (The meat was not in the least degree digested.)

It will be observed that during sleep the sound continues to travel forwards with considerable regularity. In all 12 cm. of the fistulous intestine is traversed in forty minutes.

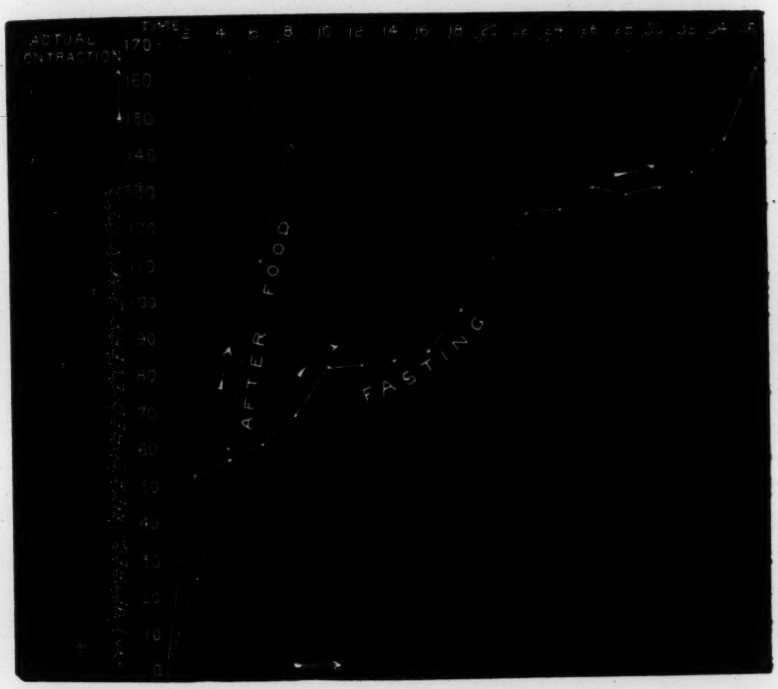
The experiments of Ranvier have shown that if a travelling sound which is passing down the œsophagus be hindered in its transit by traction being exerted upon a thread connected with it, the contractions become inoperative, *i.e.*, fail to forward the body, and then cease, the muscular tube becoming "accustomed" to its presence. The same holds good if we introduce a moderately large sound into the intestinal fistula and allow it to travel. The fact of its introduction and presence favours its passage onwards for a certain distance with considerable rapidity; but if gentle traction be made on the thread after a period of active but futile contraction,* rest occurs and no advancement takes place till a new phase of activity develops itself.

* These contractions are local. I have hardly ever seen them pass the body whose presence is to a large degree the cause of their occurrence and travel to a lower part of the intestinal tube.

It is thus that during a fasting condition, the sound being unrestrained, the first part of the fistulous intestine is traversed rapidly; but that afterwards a marked diminution in speed occurs, whilst with a smaller sound moving freely, or a larger sound restrained for a time, the rapid initial peristalsis would not be observed.

In illustration of this point I give the notes of an experiment.

Experiment III, Chart II. Experiment VI, Chart V.



Experiment III (Chart II and Curve II).—0′. Introduced large oblong sound (metal capsule, weight 3·4 grams, 18 mm. long by 9 mm. greatest breadth) into upper fistulous opening. Sound allowed to travel, no traction being made upon it but that occasioned by the weight of the pen (2 grams).

Experiment III, Curve II.



Reduced scale = $\frac{7}{39}$ths.

1′. Active propelling peristalsis; sound has travelled 2 cm. into intestine.

2′. Continuation of active peristalsis; sound advanced in all 5·2 cm. (At this time effect due to the introduction of sound passes off, and advancement occurs more slowly; local non-propulsive movements taking place from time to time.)

8′. Has advanced in all 7·4 cm.

14′. Has advanced in all 8·2 cm.

18′. Advancement has been slow; is now 10 cm. from upper opening of gut. At this point contraction becomes more active.

25′. 12·8 cm. traversed; peristalsis succeeded by a pause.

29′. 12·9 cm.; pause in contractions.

35′. 16·2 cm.; active propulsive peristalsis reappeared. Capsule expelled from lower fistulous opening.

The total time occupied in the passage of the sound through 16·2 cm. of fistulous intestine was 35′.

Effect of Feeding.—I will now turn to the effect produced upon the fasting intestine by the introduction of food into the stomach. The method of experimentation will be at once apparent.

A fixed or travelling sound was introduced through the fistula, and the condition of the fasting intestine, as regards its movement, was recorded. At a noted time the dog, whose head had been kept covered with a cloth, had a series of pieces of meat given to it which it rapidly swallowed without changing its position on the table. (The covering of the head was a necessary precaution which I adopted in almost all the observations in order that this experimental procedure should not be associated in the mind of the animal with the administration of food only; for if the dog saw meat when in a condition of hunger, or had its proximity suggested in any way, it was sufficient to produce an active though frequently transitory contraction in the fistulous gut.)

The notes of the following experiment which was made when the animal was showing unmistakeable indications of hunger, will illustrate the modification in an already existing peristalsis on the introduction of food into the stomach.

Experiment IV (Curve III).—

0′. Introduction of large sound, used in the previous experiment.

1′. Active peristalsis has commenced.

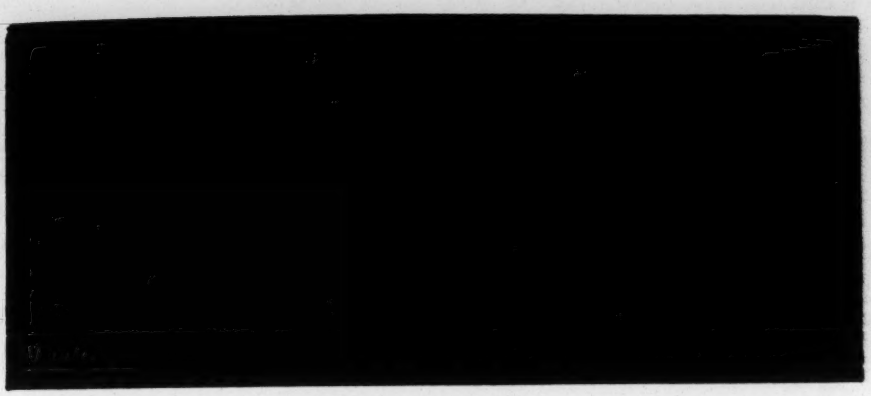
3′. Propulsive movements alternate with local or non-propulsive.

5′. The animal was somewhat excited by the movement of a plate on to the table on which it was lying.

7·5′. About 50 grams of meat were presented to the dog in pieces and rapidly swallowed.

10′. Intestine quiet, except for faint local movements around the sound.

Experiment IV, Curve III.



Reduced scale = $\frac{1}{5}$th.

11′. Active peristalsis has developed, the sound passing with great rapidity onwards; so little relaxation occurs that the curve has the appearance of steps at some parts.

16′. Sound lying within lower fistulous opening through passage of which it is retarded by thick muscular and cutaneous walls, which somewhat overhang in this position.

In order to render the alteration in the speed of peristaltic propulsion more apparent, I formulate from the experiment the distance traversed by the sound in every two successive minutes.

Time after introduction of sound in minutes..	Distance accomplished by sound in centimetres.	
	Fasting.	Fed.
0	0	—
2	1·6	—
4	1·0	—
6	2·2	—
8	0·5	—
10	..	0·5
12	..	4·3
14	..	3·1
16	..	2·2

Total for 8 minutes before food..... 5·3 cm.
„ after „ ..... 9·7 „

If we employ a rapidly revolving drum, the time elapsing between the introduction of food into the stomach and the occurrence of con-

traction is easily estimated. It is then found that the intestine from a condition of entire rest or very faint contraction frequently passes with great rapidity to a state of powerful and regular propulsive contraction.

If the movement be registered by a stationary sound connected with a manometer, the course of the contractions occurring at the part of the fistulous intestine surrounding the bladder is recorded. It will be observed then that the individual contractions develop gradually to a maximum, and thereafter decline rapidly, their rhythm or rate of recurrence being very constant throughout.

Experiment V, Curve IV.—Administration of Meat to Fasting Animal during Absence of Contraction of Intestine (fixed sound and mercurial manometer).



Experiment V (Curve IV).—Dog fasting, sound, moderately distended with water, introduced 3 cm. into upper fistulous opening. The contraction round the sound as a result of its introduction and extension occurred regularly at the rate of ten for 1′. After spontaneous movement had ceased the smell of the meat produced excitement with distinct contractions, which did not entirely disappear for 3′ 30″.

0′ 0″. When rest had again intervened, a small piece of meat weighing 5 grams was given.

1′ 30″. Another piece of the same size. Both of these were swallowed the instant they were presented.

1′ 45″. Contraction commenced at the rate of seven in 40″.

3′ 20″. Active contractions reached their maximum. Some tonic contraction is still seen between active compression of the sound, so that the curve remains elevated above the abscissa.

4′ 16″. The active contractions which had been declining markedly for 30″ came to an end, and were succeeded by a series of short waves which lasted for 1′ till at

5′ 16″. The intestine came to rest.

Five minutes later a piece of meat was again offered, faint movement being present at the time, the maximum of the powerful contractions was reached in 40″, in exact correspondence with the former experiment. In the first result there were nineteen well-

marked contractions, having approximately a maximal value; in the second there were seventeen.

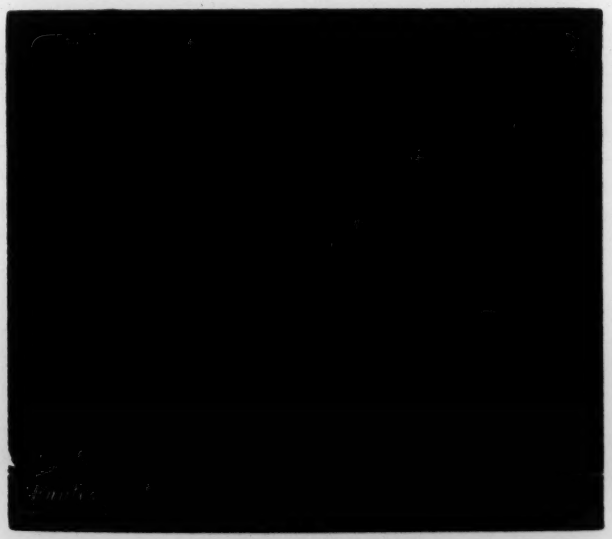
After the lapse of another five minutes, the administration of another piece of meat failed to arouse any contractions.

Immediately after a full meal a travelling sound introduced into the upper fistulous opening is forwarded by a powerful and regular peristalsis to the lower opening and promptly expelled, no marked halt occurring during the passage. Not only do the local contractions manifest themselves to a striking extent, but writhing, &c., rolling movements ("Roll-Bewegungen") of the fistulous intestine are also observed. It is during this period that the most rapid propulsion of the sound occurs, relaxation between the contractions is very imperfect, the sound being held without marked retrogression till the next powerful wave drives it onwards.

The presence of abundant peristalsis in the small intestines generally is evidenced by borborygmi and movement communicated to the abdominal parietes. The increase of secretion which is extruded from the lower fistulous opening, and to a much smaller extent from the upper opening, is very apparent.

I select two experiments for illustration.

Experiment VI, Curve V.—Fifteen minutes after full Meal.



Reduced scale = $\frac{4}{11}$ths.

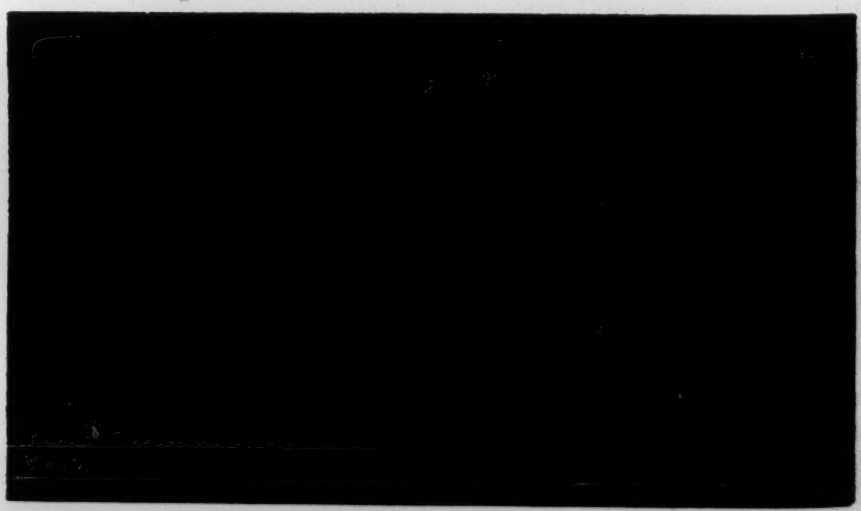
Experiment VI (Chart V (broken line, diagrammatic) and Curve V). —Taken 15′ after a full meal (contrast this with Observation III made on the same day before the meal). Large capsule employed. The animal slept the whole of the time.

0′. Sound introduced; pendulum movements for first minute.
1′. „ advanced 7 mm.
2′. „ „ 25 „
3′. Movement of all intestine.
4′. Sound advanced 60 mm. The propulsion of the sound greatly accelerated.
5′. Contractions eleven to the minute; relaxation between. Waves very imperfect.
6′. Sound advanced 111 mm.
6′ 30″. Sound advanced 142 mm. No marked pause takes place in the occurrence of the waves. A maximum rapidity of 38 mm. in the minute is attained towards the end of the curve.

Contrasting this result with Observation III gives the relative speed of the progressive peristalsis in the two cases.

Time after introduction of sound in minutes.	Sound travels	
	Before food (hunger).	15 minutes after food.
	cm.	cm.
2	5·3	2·5
	The progressive peristalsis stimulated by introduction of sound.	
4	0·2	3·5
6	0·4	5·1
7	..	3·1
8	0·8	—

Experiment VII, Curve VI.—See Chart VI.



Reduced scale = $\frac{4}{11}$ths.

The last experiment was made with a metal capsule, and in order to show that the nature of the sound has no marked influence upon the rapidity of propulsion immediately after food has been introduced into the stomach, I shall quote the results obtained when the meat sound employed in Experiment II was substituted for the metal capsule. It will be seen that whether the body to be forwarded by peristaltic movement be incompressible or soft and plastic, the result obtained is practically the same.

Experiment VI (Chart and Curve VI).—The dog had finished a full meal six minutes before introduction of the sound.

0′. Introduce meat sound deeply through the upper fistulous opening. Animal sleeping.

1′. 11 mm. traversed.

2′. 35 „ „ } Very strong peristalsis with some "roll" movements of the loop of intestine.

3′. 62 „ „

4′. 76 „ „

5′. 92 „ „ Animal awake, deep respirations.

6′. 103 „ „

7′. 113 „ „

8′. 119 „ „ } Slight delay towards lower mouth of fistulous tract, the last 2 cm. of which seems occasionally to act more feebly than the rest.

9′. 123 „ „

9′ 30″. 131 „ „

Tabulated with the results of Experiment II (Chart I) the speed of propulsion is relatively the following :—

Time after introduction of sound in minutes.	Sound travels	
	Before food (hunger).	Six minutes after food.
	cm.	cm.
0	0	0
2	0	3·4
4	0·5	4·3
6	1	3·4
8	1	2·6
10	0·3	2·3
12	0	—
14	0·1	—
16	0·6	—

The sound traversed 11·5 cm. of the fistulous intestine of the fasting animal in 46′.
„ „ „ fed „ 6·5′.

During the later stages of digestion, *i.e.*, one and a half to three hours after a full meal, the transmission of a solid body through the

fistulous intestine is fairly regular and rapid. There is, however, more tendency to occasional pauses and to the development of local non-propulsive movement which often inaugurate a phase of peristaltic activity. Four or five hours after a meal and until the recurrence of hunger—period of repletion—the progression of a solid body through the fistula is at its slowest, long pauses being frequently observed without the occurrence of any propelling contractions.

Even when peristalsis is occurring it is slow, and has much of a forward and backward character, complete relaxation behind the sound succeeding an active local contraction.

A short tabulated statement of the results of several experiments dealing with the speed of transmission of a solid body during fasting (hunger), digestion, repletion, &c., may be found useful for purposes of comparison. The average time for each centimetre of the intestine traversed is not of course of absolute value.

Condition of animal.	Length of fistulous intestine traversed.	Total time occupied.	Average time for each cm. of intestine traversed.	Sound employed.
	cm.	′ ″	′ ″	
Fasting (*a*) ..	15·25	20 30	1 30	Large metal capsule.
„ (*b*) ..	9	15 0	1 40	Small „
„ (*c*) ..	15·5	27 30	1 48	Large „
„ (*d*) ..	16	33	2	Cork, 9 mm. × 14 mm.
„ (*e*) ..	15	38	2 30	Large metal capsule.
„ (*f*) ..	12	40	3 20	Lean meat.
After exercise (*g*) ..	13·5	16 30	1 12	Large metal capsule.
Digestion (*a*)..	14·2 (15′ after food.)	6 48	0 26	Large metal capsule.
„ (*b*)..	15·5 (8′ after food.)	8 48	0 36	Lean meat.
„ (*c*)..	14·75 (2 h. 34 m. after food.)	13 36	0 54	Large metal capsule.
Repletion (*a*)..	7·5 (4 hrs. after meal.)	76	10	Large metal capsule.

(The transit in the last case I have quoted is one of the slowest I have observed. During the first 10′ there was a progression of 1·5 cm., preceded by a pause. Local non-propulsive movements followed by peristalsis succeed in 20′ (4 cm. in 9′); passage of the next 3 cm. took 36′.)

Some variation in peristalsis according to the sound employed became

evident after a few experiments had been made with each. The introduction of the larger metal sound (9 × 14 mm.) was usually followed by a well-marked peristalsis occurring immediately afterwards (Experiment III), lasting for 1′ to 3′, and propelling the sound 2 to 5 cm. into the fistula of the fasting animal. With the cork sound of equal size, but lesser weight, the effect was the same; with the narrow capsule (6 × 14 mm.) and the meat fragment, this primary peristalsis of introduction was much less marked. After the period of activity had passed, the time occupied in the transit through the entire length of the fistula appeared to be closely similar in the larger and smaller bodies; in other words, those of greater size which at first excited motion soon ceased to do so, the intestine becoming "accustomed"—as the œsophagus does—to their presence.

When a phase of activity is present, as during the time immediately succeeding the introduction of food into the stomach, all evidence that larger bodies contribute by their size to a more rapid propulsion through the fistula, is lost. (If there were greater variation in the diameter of the sounds employed than existed in those used in these experiments, it is possible that some slight exceptions to this statement might be found.)

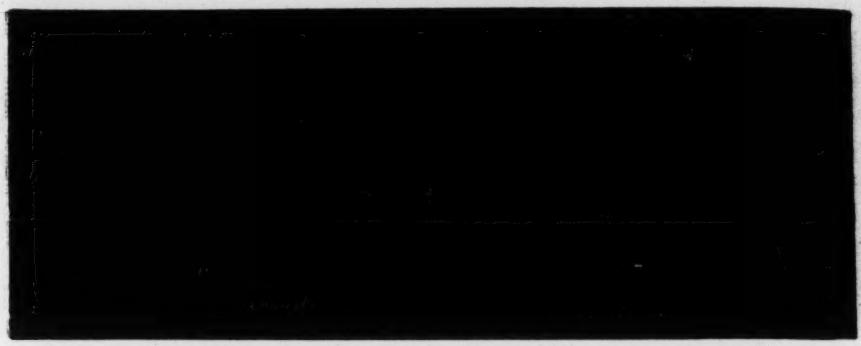
Changes in Character of Peristaltic Contractions.

It is not my purpose at the present time to enter into a minute examination of the character of individual peristaltic contractions occurring under various circumstances, although I have alluded to this question very briefly on page 214. As, however, the consideration of peristalsis, stimulated by the introduction of a large sound into the fistula, has been brought under notice, I can scarcely omit contrasting the nature of the waves produced in this manner (and which are very similar to those occurring in all portions of the fasting intestine), and the digestive peristalsis occurring in the same animal half an hour after a full meal.

It is found on registering them upon a cylinder having a medium velocity, that the contractions occurring in the first (fasting) condition are much more numerous in a given time, frequently nearly twice as many as in the latter; that the individual contractions have a shorter maximum which is attained more quickly, and that their relaxation is more abrupt and complete. As a result we find that in spite of the greater number of contractions the sound advances further in the same time after food than it did when food had not been recently administered. Some hours after the meal the change is even more striking, long periods of rest occurring which are from time to time broken by a few faint contractions, having a sustained maximum and an imperfect relaxation.

The quotation of one experiment will illustrate these facts sufficiently.

Experiment VIII, Curve VII.—Travelling Sound in Upper Fistulous Opening (quick drum).



a. Peristalsis on introduction of large sound (before feeding).
b. „ „ (after „).
c. „ in advanced stage of digestion.
„ Lower line indicates respiration.

Experiment VIII (Curve VII, *a*, *b*, *c*).—Dog fasting sixteen hours. Large travelling sound (9 × 18 mm.) introduced into upper fistulous opening. Record on drum of medium velocity.

0′. In 80″ succeeding introduction of sound twenty-three contractions occur (*a*). Average length of each contraction 3·3″. Total advance of sound 28 mm. Sound removed.

5′. Full meal of meat given to dog.

35′. Sound again placed in upper opening. (Slight irregularity in curves caused by tremor of animal, which often occurred for a time after meal, and to a lesser extent by respiratory movements.) In 80″ succeeding introduction of sound, fourteen contractions occur (*b*). Average length of "forwarding" peristaltic waves 5″. Average length of two contractions succeeded by a more complete relaxation occurring in the middle of tracing 3·3″.

Actual advance of sound 38 mm.

4 hours. At time of registration contractions take place at the rate of six and a half in 80″. Sound advances at rate of 10 mm. in 80″ (*c*). Contractions which are long maintained recur for 20″ to 60″, and are succeeded by pauses of several minutes' duration.

This change in the rhythm, character, and potentiality of the peristaltic contractions shows how extensive the range of adaptation of these movements to the necessities of the time must be.

In the experiment quoted we see (1), vigorous propulsion accomplished by frequent repetition of contraction; (2), more vigorous propulsion accomplished by fewer, but more sustained contractions; and (3), the leisure and occasional movement of the intestinal tube which would be favourable to active absorption.

Direction of Propulsion of Bodies introduced into the Fistulous Openings.

The result of an examination of this question experimentally may be given in few words.

Peristaltic progression in the fistulous intestine is always in the physiological direction. If the sound be placed well within the lower mouth, it is invariably rejected from this mouth. Very occasionally a sound introduced just within the upper mouth, but not far enough to be thoroughly "gripped," may be expelled by the same opening, but after it has passed 2 cm. into the fistula, this retrogression is never seen. A slight anti-peristaltic movement observed on the introduction of water into the fistula will be described below.

Two Solid Bodies in the Fistula at the same Time.

This experiment was performed with a double sound and registering apparatus. The second sound, which was introduced after the first had passed several centimetres into the fistula, was perforated for the free passage of the thread connecting the latter with its pen. It was found that the condition of peristalsis or local contraction was not necessarily common to the whole length of the fistulous intestine, and that therefore movement of one sound occurs frequently quite independently of the other which lies within a few centimetres of it. On several occasions a distinct alternation in movement was observed, one sound resting whilst the other advanced. The first sound, *i.e.*, that nearest the lower opening of the fistula, seemed usually to travel more rapidly than the other, and therefore to gain upon it. This fact may speak for a slight stimulation originated by the presence of the upper sound when at rest, favouring a more pronounced peristalsis lower down the intestinal tube. Upon this point I am seeking for further information.

The introduction of water or almost any liquid, whether cold or at the body temperature, into the upper end of the fistula through which the sound had already passed, was invariably productive of some slight anti-peristaltic movement often associated with writhing or rolling of the intestine, the result being a retrogression of 2 or 3 cm. This reversed movement, which lasted for 2′ to 5′, was succeeded by a return of propulsive peristalsis which frequently showed a marked acceleration.

Experiment IX, Curve VIII.



Reduced scale = $\frac{1}{3}$th.

Experiment IX (Curve VIII).—Injection of 5 c.c. of water at the fourth minute into the upper fistulous opening, peristalsis being present, caused on the first occasion a relaxation lasting for 4′, and on the second, between the twelfth and thirteenth minutes, for 2′, the pen falling 18 mm. After each of these injections the peristalsis became for a time more active.

Propulsion of a Sound upon which Traction is being Exerted.

If the pen were weighted with a burden of 8 or 10 grams at a time, when the sound was being passed onwards by well-developed peristaltic movement it was found that not only was the portion of intestine in which the travelling body was situated at the moment drawn towards the mouth of the fistula, but that the powerful contractions which occurred were incapable of materially forwarding the body. Even the traction caused by applying a weight of 5 grams to the pen produced much hindrance, and the spasmodic contractions induced and which had a propulsive character created a most distinct discomfort or colic. After the removal of the hindrance to effective transmission, the intestine unfolded itself, whilst the discomfort gave place to the most evident manifestations of satisfaction on the part of the dog, and active peristalsis—which was, however, soon reduced to its orginal character—took place.

In contrasting this experiment with the well-known result obtained by Mosso, who observed that the œsophagus could hold a sound connected with a falling weight which exerted a traction of 450 grams, and forward one of but little less, the rectilineal course of the tube, and the fact that it is practically fixed at both ends—in both of which respects it acts at a far greater advantage than the intestine—must be borne in mind. I possess evidence, however, which I hope to be in a position to produce shortly, that even if these unfavourable conditions are obviated, the small intestine is physiologically unfitted for the systematic propulsion of bodies upon which more than a very

moderate degree of traction is made. Fortunately this condition of traction upon a solid body already engaged in the intestinal tube can but rarely come into operation in the animal body, or it would undoubtedly prove peculiarly difficult to overcome.

Mechanical irritation of the lower mouth or upper mouth of the fistulous intestine, if persisted in, developed a powerful peristalsis which caused the rapid passage of a solid body through the fistula and its expulsion by the lower opening. Thus if the animal was induced—which it readily was—to lick the mouth of the fistula after the sound had been inserted in the upper opening, expulsion from the lower opening occurred in four to eight minutes. By moving a glass rod—passed within the mouth of the fistula—gently round or in and outwards, the same result was occasionally produced, but not with the same speed or certainty.

The consideration of the effect of electrical stimulation and of a number of drugs, upon whose action I believe new light will be thrown by this method of experimentation, I must leave to a future paper.

I have purposely avoided entering into a discussion of the physiological *rationale* of the various phenomena I have described in this communication, because they cannot find their full explanation in the examination of a fistulous animal. This explanation must be sought by experiment of another character, and although much has been rendered clear to me by the analytical research I have already made, other points are still lacking their complete elucidation. Until the latter research appears, I likewise postpone reference to the literature of the subject.

To the Royal Society, which presented me with a donation in aid of this research, and to Prof. Hugo Kronecker, of Berne, who most kindly placed his laboratory at my disposal for its accomplishment, my best thanks are due, and I gladly take this opportunity of recording them.

"On the Practical Measurements of Temperature. Experiments made at the Cavendish Laboratory, Cambridge." By H. L. CALLENDAR, B.A., Scholar of Trinity College, Cambridge. Communicated by J. J. THOMSON, F.R.S., Professor of Experimental Physics at the Cavendish Laboratory, Cambridge. Received June 9.—Read June 10, 1886.

(Abstract.)

The experiments were undertaken with a view to investigate the possibility of establishing strictly comparable standards for high temperature measurements for which the air thermometer is in-

applicable or unsatisfactory, since even the best observers using all possible precautions disagree.*

The platinum wire used was the *purest obtainable*, with an unusually high temperature coefficient, and very unalterable. If R_1 be the resistance of a given coil at 100° C., and R_0 the resistance at 0° C., and R the resistance at any other temperature, the experiments abundantly prove (1) that with proper precautions R_0 is constant to 0·01 per cent., even if the coil has been subjected for hours to a temperature of 1300° C. (2) For several different pieces of wire *from the same reel* it was found that the ratio $\frac{R_1}{R_0}$ was constant to the same degree of accuracy, and equal to 1·3460. (3) That for two *different wires* of *pure* platinum, though the values of $\frac{R_1-R_0}{R_0}$ may differ 3 per cent., yet the values of $\frac{R-R_0}{R_1-R_0}100$, that is to say, of the temperature Centigrade by platinum wire, agree to 0·1 per cent. through the range 0—600°.

The method of comparing the temperature variations of different wires is very accurate. The wires to be compared are interwound symmetrically so as to form a double screw thread on non-conducting material (*e.g.*, clay), and are heated in a vacuous porcelain tube in a gas furnace regulated to produce steady temperatures. When the temperature is steady we may assume that the mean temperatures of the two wires are accurately the same. The simultaneous resistances are measured by the Wheatstone bridge method with a sensitive mirror galvanometer and resistance boxes accurate to 0·01 per cent. The comparison of two similar pure platinum wires might be effected with even greater accuracy than 0·01 per cent. by using a wire bridge (Carey Foster's method), since the ratio of their resistance is very nearly constant (variation perhaps 2 per cent. between 0° and 600°), and all sources of error affect both wires equally. The resistance of the connecting wires at each steady temperature is measured and allowed for, and thermoelectric effects are carefully eliminated: as a precaution the insulation is also measured.

It is evident then that by adopting one wire as the standard and comparing others with it, temperature measurements may be made strictly comparable through a very wide range, since the platinum thermometer is almost universally applicable, and is usually more convenient, sensitive, and accurate than any other kind. The chief objection to its use is the need of auxiliary apparatus for accurately measuring the resistance: its great superiority lies in its wide range, its freedom from zero error, and its adaptability. A length of wire is

* See article by W. N. Shaw, "Pyrometers," 'Encycl. Brit.,' 9th Edit., vol. 20, p. 129.

cut off and disposed in whatever way is most suitable to the requirements of the particular experiment.

Comparisons were also made between the platinum pyrometer and the air thermometer at constant mass and volume up to nearly 600° C. By enclosing the fine platinum spiral inside the bulb of the air thermometer itself, errors of unequal heating were to a great extent avoided. A modified form of air thermometer was devised for the purpose, in which the air is confined at constant volume by sulphuric acid in an auxiliary gauge which is connected to the mercury manometer by vulcanised rubber tube. For the high temperature observations the bulb was made of a combustion glass tubing which does not soften till 700° C.; the linear expansion of this kind of glass was determined by a very accurate method, in which the mean temperature of the glass tube is given by a platinum wire extending down its axis. It was found that if kept for any time at about 500° C., or higher, the glass bulb is liable to capricious changes of volume amounting to about 0·1 per cent. or more; by a slight and convenient modification of the sulphuric acid gauge such changes may be determined on the principle of the volumometer and allowed for. By avoiding the permanent change the linear expansion could be represented by the formula $\frac{l}{l_0}=1+\cdot 00000680\,t+2\cdot 7\,.\,10^{-9}\,t^2$, with a mean error of 0·2 per cent. per single observation. Among other sources of error affecting the air thermometer at these temperatures we may mention surface condensation. It seems certain that the surface air film varies with the temperature, and when completely removed by exhaustion at a high temperature does not recover its normal state for some time, so that the zero pressure may decrease upwards of a millimetre of mercury in a week. The investigation of this is, however, still incomplete.

The results of all the observations with three different instruments are well represented by the formula

$$\mathrm{R}=e^{\frac{\alpha t}{1+\beta t}} \qquad \ldots\ldots\ldots \qquad (e)$$

where t is temperature Centigrade by air thermometer, and the values of the constants are

$$\alpha=0\cdot 0034259, \qquad \beta=0\cdot 0015290.$$

Between 0° and 200° C. the curve (e) is very nearly a straight line, $\mathrm{R}=1+0\cdot 003460t$, as it has a point of inflexion at about 80° C. The mean deviation of an observation between 400° and 600° from this curve is about 2·5° C., but it would be rash to use the formula (e) outside the range covered by the experiments: it is for all reasons far

preferable to use $t=\frac{R-R_0}{R_1-R_0}100$, and to call the temperature so calculated temperature by platinum wire, as this involves no assumption and different wires nearly agree. It is from a thermometric point of view that the method above described of comparing different wires with the standard is most valuable: but it might also be employed to form very accurate comparative tables of the change of resistance with temperature for different metals referred to platinum as a standard. Such tables would, however, be of little use apart from the actual specimens of metal employed, except in so far as they tended to elucidate the general phenomenon of the variation of resistance with temperature. With this view a comparison was made between platinum and iron, the resistance variation of which metal appeared likely from previous observations to differ widely from that of platinum. The curve deduced resembled the steam pressure curve, and suggested the formula*

$$R=e^{\frac{\alpha t}{1+\beta t}} \quad \ldots\ldots\ldots \quad (e)$$

A formula of the same type was found to satisfy the air thermometer observations for platinum better than any other. The corresponding differential equation is $\frac{d}{dt}t^2\frac{d}{dt}(\log R)^{-1}=0$. If R_1, R_2 be any two observed resistances of the platinum coil, and dashed letters express corresponding quantities at the same temperatures for iron, we have from (e), $\frac{\alpha}{\log R_1}-\frac{\alpha'}{\log R'_1}=\frac{\beta-\beta'}{\log e}=\frac{\alpha}{\log R_2}-\frac{\alpha'}{\log R'_2}$, by equating temperatures. The comparison observations are tested and found to satisfy this equation of condition; from which we may deduce the ratios $\alpha:\alpha':(\beta-\beta')$. Assuming the values of α and β for platinum to be $\alpha=0{\cdot}0034259$, $\beta=0{\cdot}0015290$, we find for iron the values $\alpha'=0{\cdot}0045657$, $\beta'=0{\cdot}0007767$.

The following table containing some of the observations taken by this method, *with the temperature varying*, shows the capabilities of the method and the sensitiveness of the platinum spiral.

The time observations show the rate of variation of the temprature. The resistance calculated by formula (a) $\frac{R}{R_0}=1+0{\cdot}003460t$, shows the extent of the deviation from the straight line.

The difference column shows how nearly the observations agree with formula (e), and the smallness of the probable error of a single observation.

* Regnault, 'Paris, Acad. Sci. Mém.,' vol. 21, 1847, p. 619.

Table I.

No. of observation.	Time.	Temperature Cent. by air thermometer.	Resistance value observed.	Resistance calculated by empirical formulæ.		Difference between (*e*) and observed resistance	
				(*a*.)	(*e*.)	In ohms.	In degrees Cent. error.
	hrs. min.						
4	11 39	348°·6	2·1788	2·2062	2·1793	+0·0005	0°·16
5	11 42	346·0	2·1709	2·1972	2·1711	+0·0002	0·06
6	11 44	342·4	2·1591	2·1847	2·1595	+0·0004	0·13
7	11 45½	339·1	2·1493	2·1733	2·1492	−0·0001	0·03
8	11 47	336·1	2·1394	2·1629	2·1395	+0·0001	0·03
9	11 49	331·8	2·1239	2·1480	2·1235	−0·0004	0·13
10	11 52	323·8	2·1003	2·1203	2·0998	−0·0005	0·16
11	11 54	317·8	2·0806	2·0996	2·0807	+0·0001	0·03
12	12 0	299·6	2·0217	2·0366	2·0213	−0·0004	0·13
13	12 3	293·6	2·0020	2·0159	2·0020	+0·0000	0·00
14	12 8	281·5	1·9629	1·9740	1·9625	−0·0004	0·13

Table II.

Comparison of Platinum and Iron.

Temperature Cent.			Resistances reduced to 1 at 0° C.			Differences between observed and calculated.	
By air thermometer formula (e) $\frac{1}{t}=\frac{\alpha \log e}{\log R}-\beta$.	By platinum wire formula (a) $t_{pt}=\frac{R-R_0}{R_1-R_0}100$.	By iron wire formula (a') $t_{fe}=\frac{R'-R'_0}{R_1-R'_0}100$.	Simultaneous pairs observed. Platinum.	Simultaneous pairs observed. Iron.	Iron calculated by equation (e') $\log R'=\frac{a't \log e}{1+\beta' t}$.	Expressed in percentage of R′.	Expressed in degrees of temperature Cent.
0°	0°	0°	1	1	1	0	0°
35·57	35·41	32·8	1·1225	1·1732	1·1724	0·07+	0·14+
99·79	99·79	99·79	1·3453	1·5236	1·5263	0	0
173·3	173·2	190·3	1·5990	2·0036	2·0057	0·10−	0·15−
269·2	266·5	335·4	1·9220	2·7692	2·7667	0·09+	0·11+
364·1	355·0	506·4	2·2283	3·6715	3·6636	0·21+	0·20+
470·9	449·4	726·0	2·5548	4·8396	4·8342	0·11+	0·09+
586·3	544·5	998·4	2·8840	6·2667	6·2775	0·18−	0·14−
640·9	587·1	1148·2	3·0314	7·0566	7·0576	0·01−	0·01−

Table III.

Comparison of Platinum Wires.

Temperature Centigrade.			Resistances reduced to 1 at 0°. Simultaneous pairs observed.		Ratio of pairs of resistances.	Mean coefficients between 0 and t, assuming standard constant $=0{\cdot}003460$.	Difference of mean coefficients per cent.
By standard wire formula (α) $t=\frac{R-R_0}{R_1-R_0}$.	By compared wire formula (α) $t=\frac{R'-R'_0}{R'_1-R'_0}$.	Difference in degrees Centigrade.	Standard wire.	Compared wire.			
557°·80	558°·56	−0·76	2·9300	2·8790	0·98260	0·003369	2·63
338·15	338·74	−0·59	2·1700	2·1395	0·98590	0·003369	2·63
99·15	99·15	0	1·3429	1·3334	0·99289	0·003364	2·78
34·68	34·66	+0·02	1·1200	1·1166	0·99696	0·003362	2·83
18·78	18·76	+0·02	1·0650	1·0631	0·99824	0·003360	2·90
0	0	0	1	1	1	—	—

The chief difficulty of the comparison is that of protecting the iron wire from alteration when it is maintained at a red heat for some time. The following Table, II, gives some observations selected at random from a long series in which the zero variation of the iron did not exceed 0·05 per cent. The observations were mostly taken on different days; and the resistances observed at the air temperature in the intervals show the direction in which the correction for zero variation should be applied, but not its amount. It is noteworthy that in all cases the correction for this would tend to reduce the small differences between the formula (e) and the observations. As it is, the mean difference is only 0°·1 C.

It does not appear that any other equally simple and convenient formula could be found representing the observations equally well.

Treating the observations on the comparison of two platinum wires in precisely the same way, we shall find for the values of the constants, assuming $\alpha\beta$ as before for the standard wire—

$$\left.\begin{array}{ll} \text{Standard wire } \alpha = 0\cdot0034259. & \beta = 0\cdot0015290 \\ \text{Compared wire } \alpha' = 0\cdot0033332. & \beta' = 0\cdot0015256 \end{array}\right\}$$

so that the values of β are almost identical.

If $\beta=\beta'$ we have the relation between the resistances at the same temperature

$$\frac{d\mathrm{R}}{dt} \Big/ \frac{d\mathrm{R}'}{dt} = \frac{\alpha\mathrm{R}}{\alpha'\mathrm{R}'}, \text{ or } \frac{\log \mathrm{R}}{\log \mathrm{R}'} = \frac{\alpha}{\alpha'}.$$

It is intended to continue the experiments at higher temperatures, and to apply the platinum thermometer to other investigations.

"The Determination of Organic Matter in Air." By Professor Thos. Carnelley, D.Sc., and Wm. Mackie, M.A., University College, Dundee. Communicated by Sir H. E. Roscoe, F.R.S. Received June 10. Read June 10, 1886.

The only methods hitherto proposed for determining the amount of organic matter in air are the two devised by the late Dr. Angus Smith ("Air and Rain"). According to the first of these methods, a definite quantity of the air to be examined is slowly bubbled through a dilute solution of potassium permanganate of known strength until it is fully or considerably bleached, and in the latter case the amount of undecomposed permanganate determined by oxalic acid. In the second method a known volume of air is bubbled through distilled water, and the latter examined for free and albuminoïd ammonia by Wanklyn and Chapman's process for water analysis.

These methods are open to one or more of the following objections:—

1. The time required for a single determination is *very considerable*, and necessarily varies with the amount of organic matter present.

2. There is great uncertainty as to whether the organic matter has been fully absorbed and acted on by the permanganate in the first method, or absorbed by the water in the second.

3. It is difficult to determine the exact point of full bleaching, or to estimate by oxalic acid very small degrees of partial bleaching.

4. The methods are inapplicable (chiefly on the score of time and of the extent and complication of the apparatus required) in circumstances and places where such determinations are most desirable. Analyses, especially by the second method, cannot be completed on the spot, except in very rare cases, so that unless a large quantity of apparatus be taken, several consecutive determinations in a series of buildings or rooms is impossible (*cf.* De Chaumont, 'Roy. Soc. Proc.,' vol. 23, 1875).

5. The results obtained by the method proposed below show that very considerable variations in the organic matter are sometimes liable to occur within the period of determination required by Dr. Angus Smith's method.

By the new process, which it is the object of the present paper to describe, the above difficulties are in a great measure overcome. The special advantages we claim for it are:—(1.) Rapidity and simplicity of execution. (2.) A higher probability, though not absolute certainty, that the organic matter is fully absorbed. (3.) A more general applicability.

The *principle of the process* is the same as that of Angus Smith's first method, viz., reduction of potassium permanganate. It differs, however, from Smith's method, more particularly in the mode of estimating the amount of reduction. This consists in determining *colorimetrically*, by comparison with a standard, the *fractional* bleaching effected in a given volume of air.

Method.—The solution of permanganate used is of $\frac{N}{1000}$ strength, of which 1 c.c. = 0·008 mgrm. of oxygen = 0·0000056 litre of oxygen at 0° and 760 mm. It is usually kept of $\frac{N}{10}$ strength, and diluted as required, about 50 c.c. of dilute sulphuric acid (1 to 6) being added to each litre of the weak solution.

For the collection of the samples of air large well-stoppered jars of about 3·5 litres capacity are used. The jars are first rinsed out with a little standard permanganate, and when not in use some of the solution is always left in them, so as to ensure complete cleanliness from any reducing substance. Before use the jars are drained, and the

sample of air collected by pumping out the contained air with a small bellows, and allowing the air to be examined to flow in. 50 c.c. of the standard permanganate are next run into the jar, which is then tightly stoppered and well shaken up for at least five minutes. 25 c.c. of the permanganate are afterwards withdrawn by a pipette and placed in a glass cylinder holding about 200 c.c., 25 c.c. of the standard solution being placed in a similar cylinder for comparison. Both are next diluted up to about 150 c.c. with distilled water, and allowed to stand for ten minutes, after which the tints in the two cylinders are compared. Standard solution is then run in from a burette, until the tints in both cylinders are of the same intensity; usually from $\frac{1}{2}$ to 6 c.c. are required.

The amount of solution added from the burette is a measure of the bleaching effected by the known volume of air on half the permanganate employed. This multiplied by 2 gives the total bleaching.

The results may be expressed either in terms of the number of c.c. of the $\frac{N}{1000}$ bleached by 1 litre of air, or, as we prefer, by the number of volumes of oxygen required to oxidise the organic matter in, say, 1,000,000 volumes of air; *e.g.*, 25 c.c. of solution from a 3·5 litre jar, in which 50 c.c. had been used, required 3 c.c. of the permanganate to bring it up to the standard, or the whole 50 c.c. would have required $3\times2=6$ c.c. This represents the number of c.c. of standard permanganate bleached by $3500-50=3450$ c.c. of air; consequently $\frac{6}{3{\cdot}45}=$ 1·74 c.c. is the bleaching effected by 1 litre of air. But 1 c.c. of $KMnO_4=0{\cdot}0000056$ litre of oxygen; $\therefore$ 1·74 c.c. $KMnO_4=0{\cdot}0000056\times1{\cdot}74=0{\cdot}0000097$ litre of oxygen is required to oxidise the organic matter in 1 litre of air, or 9·7 volumes of oxygen to oxidise the organic matter in 1,000,000 volumes of air.

Correction for temperature was not considered necessary, as it falls within the limits of experimental error. It requires about twenty minutes to collect the samples and complete the analysis. Scrupulous cleanliness is, of course, necessary in all the operations.

We have examined many hundreds of samples of air by this method, and in this large experience of it have found it to be a very convenient and ready process. We believe it to be as accurate as is possible in the present state of the knowledge of this subject. Duplicate analyses of the same air give very concordant results, as evidenced by the examples given below. We are nevertheless fully conscious that objections may be taken to the method on the following grounds:—(1.) That it does not directly estimate the organic matter, but only measures the amount of oxygen required to oxidise either the whole, or more probably only a portion of it. (2.) That the permanganate acts upon various matters in the air, besides the organic matter,

such as sulphuretted hydrogen, nitrous acid, sulphurous acid, &c. (3.) That the organic matter in air is of various kinds, and that consequently the permanganate will most probably be selective in its action. Our knowledge on this point, however, is so defective that no definite conclusion is possible in regard to it. (4.) There is no satisfactory means of checking the results, the only method being to make duplicate determinations of the same air. This test, such as it is, the method stands extremely well, as will be seen from the results given below, which are taken quite promiscuously from a large number of examples :—

Organic Matter.

	Vol. of O required to oxidise the O.M. in 1,000,000 vols. of air.			
	1st Determination.	2nd Determination.	Mean.	
Outside air (Dundee)	9·0	8·9	8·95	Immediately after rain.
„ „	12·0	11·5	11·85	No rain during day.
„ „	10·0	10·2	10·1	Heavy rain, with wind.
„ „	8·6	8·1	8·35	Rain shortly before.
„ (Perth) ..	2·0	1·6	1·8	Strong wind and rain.
„ „	2·0	1·5	1·75	Strong wind, rain at intervals.
„ „	2·4	2·0	2·2	Storm shortly before.
„ „	4·8	4·6	4·7	Fine.
Class room (Dundee)	10·5	8·8	9·65	Unoccupied, but just after dusting.
„ (Perth) ..	7·6	7·8	7·7	29 present for one hour.
„ „	4·0	5·0	4·5	31 „ „
Small room	11·9	11·4	11·65	Unoccupied, but with two gas jets burning for 15 minutes.
„	12·9	13·0	12·95	With one person and one gas jet for 20 minutes.
„	17·2	17·0	17·1	Ditto after one hour and 40 minutes.
„	20·0	20·5	20·25	Ditto on another occasion.

(5.) The uncertainty that the permanganate exerts its full action in a cold acid solution of such dilution as that recommended above. The method, however, does not claim to give absolute, but only relative results.

In the general lack of better methods we trust that the modification of the permanganate process we have described will be found of great value, on account of its extreme simplicity and rapidity of execution,

and as giving under the circumstances the greatest attainable accuracy. From our large experience of it, under a great variety of conditions, we believe that, notwithstanding its more or less liability to some or all of the above objections, the method gives results which are perfectly reliable for purposes of comparison.

Conclusions.—The following conclusions are deduced from a large number of analyses. The individual data are usually the mean of two duplicate determinations.

(1.) The quantity of organic matter in outside air varies considerably, within certain limits, from day to day, and from hour to hour on the same day.

Variations from day to day are subject to the conditions of the weather.

(*a.*) It has been found somewhat less immediately after or during rain or snow, thus:—

	No rain or snow.			
	No. of cases.	Lowest.	Highest.	Average.
Organic matter (O required per 1,000,000 vols. of air)	19	1·6	15·8	7·9
Carbonic acid (per 10,000 vols. of air).....	15	2·2	5·4	3·86

	Just after or during rain or snow.			
	No. of cases.	Lowest.	Highest.	Average.
Organic matter (O required per 1,000,000 vols. of air)	19	1·8	13·3	7·3
Carbonic acid (per 10,000 vols. of air).....	11	2·4	5·6	3·95

(*b.*) The highest results of all were obtained on foggy nights, *e.g.*, 15·7, 17·0. High results were also obtained during a slight drizzling rain, accompanied by mist.

(2.) A close connexion is observed between the amount of organic matter present in air and the combustion of coal. It is lowest in the middle of the night, rather higher in the morning, and considerably

higher in the middle of the day, and higher still towards evening, after which it decreases thus:—

	Night, 8 P.M.—5 A.M.			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	9	2·2	6·7	3·9
Carbonic acid (per 10,000 vols. of air).....	9	3·0	5·6	4·1

	Morning 5 A.M.—10 A.M.			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	4	3·2	6·5	4·9
Carbonic acid (per 10,000 vols. of air).....	9	1·8	4·1	2·9

	Mid-day 10 A.M.—3 P.M.			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	12	1·6	15·7	7·9
Carbonic acid (per 10,000 vols. of air).....	14	2·2	4·9	3·4

	Evening 3 P.M.—8 P.M.			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	25	2·6	16·2	9·1
Carbonic acid (per 10,000 vols. of air).....	20	1·9	5·3	3·5

The above results refer only to ordinary week days, and do not include Saturday and Sunday, which are not normal days in a large town. On several occasions an important decrease was noted towards the noon of Saturday, no doubt due to the stoppage of the boiler fires at the mills and factories. This is shown by the following numbers, which include all the cases observed:—

1885, November 7th.		November 21st.		November 28th.	
Time.	Organic matter.	Time.	Organic matter.	Time.	Organic matter.
10·30 A.M.	8·9	11·30 A.M.	12·7	10·30 A.M.	10·2
12·15 P.M.	8·1	1·30 P.M.	10·6	12·15 A.M.	8·3
1·15 P.M.	8·3	—	—	—	—

A relation to the products of the imperfect combustion of coal is also indicated by the much higher averages obtained for outside air in the centre of Dundee, as compared with the suburbs and with Perth, thus:—

	Dundee (town).			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	46	1·7	16·8	8·9
Carbonic acid (per 10,000 vols. of air).....	32	2·2	5·6	3·9

	Dundee (suburbs).			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	10	0	5·6	2·8
Carbonic acid (per 10,000 vols. of air).....	5	1·7	3·5	2·8

	Perth (outskirts).			
	No. of cases.	Lowest.	Highest.	Mean.
Organic matter (O required per 1,000,000 vols. of air)	5	1·7	4·5	3·1
Carbonic acid (per 10,000 vols. of air).....	3	2·9	3·5	3·1

(3.) A relation of organic matter to carbonic acid in outside air shows, so far as the tabulated results go, a high carbonic acid accompanied by a high organic matter, and *vice versâ*. This, however, is by no means invariably the case, and is in fact only evident on comparing the averages of a large number of cases. To show this all the determinations which have been made in outside air were divided into four groups, according to the quantity of organic matter present, and the averages of the corresponding carbonic acid found, as in the following table:—

Vols. of O required to oxidise the organic matter in 1,000,000 vols. of air.	Average carbonic acid in 10,000 vols. of air.			No. of determinations.
	Lowest.	Highest.	Average.	
0 to 2·5	too small to estimate.	5·6	2·8	20
2·5 to 4·5	1·8	4·9	3·0	20
4·5 to 7·0	2·0	5·4	3·2	20
7·0 to 15·8	3·7	4·9	3·7	20

(4.) The organic matter in outside air has a far wider range of variations than carbonic acid. For whereas the carbonic acid seldom passes beyond the limits of 2 to 6 vols. per 10,000, the organic matter varies from a quantity too small to estimate to as much as will require for oxidation about 16 vols. of oxygen per 1,000,000 vols. of air.

It is also subject to more rapid fluctuation in its value than carbonic acid.

(5.) The combustion of gas does not appreciably increase the amount of organic matter in air. This was proved by burning gas in a small practically air-tight room (which had previously been well ventilated, and the carbonic acid and organic matter determined),

for a certain length of time, and then making a determination of the carbonic acid and organic matter in the air thus vitiated. The following is an example of a number of experiments all giving similar results:—

	CO_2 per 10,000 vols.	Organic matter (vols. of O required per 1,000,000 vols. of air).	
Before gas was burnt	4·3	10·6	Each of these is the mean of two nearly concordant experiments.
After the gas had been burning 15 minutes.....	11·0	11·8	
After the gas had been burning 30 minutes.....	14·8	11·8	

The combustion of gas may therefore be considered too perfect to produce any appreciable effect. What result is obtained may safely be set to the credit of sulphurous acid.

The above remarks apply to Dundee gas, which is considered to be exceptionally pure and free from sulphur.

(6.) *Combustion of Oil Lamps.*—The effect of burning oil lamps is much more marked than that of the combustion of gas. This was determined in a manner similar to that in the case of coal-gas, the experiments being carried out in the same practically air-tight room as before. The following will serve as an example:—

	Organic matter (O required per 1,000,000 vols. of air).	
Before burning of oil lamps	8·7	Each of these is the mean of two very closely concordant determinations.
After one lamp had been burning half an hour, the lamp was found to be smoking slightly ..	12·3	
Ditto after one hour, lamp found burning clear ..	14·6	
After the first lamp had been burning one and a half hour, and a second lamp half an hour, the second lamp was found smoking slightly	16·7	
Ditto after first lamp had been burning two hours, and the second one hour, both lamps found burning clear	18·1	

In all cases the lamps were burning paraffin oil and were turned on as full as possible without smoking.

(7.) *Respiration.*—Respired air gives a higher result than unrespired air at the same time, though much less than was anticipated.

The effect of respiration was determined as follows:—The observer worked in the small air-tight room previously referred to. This was first ventilated with outside air and then closed, and at the end of

definite intervals the carbonic acid and organic matter present in the air of the room were determined, the analyses being made in the room itself so as to avoid opening the door. The room being unprovided with outside light, one gas jet was kept burning during the whole of the experiments, but the effect of this on the organic matter may be neglected, for, as previously shown, the combustion of coal gas does not appreciably increase the amount of organic matter. The results obtained are shown below:—

		Outside air.	After 20 minutes.	After 30 minutes.	After 60 minutes.	After 100 minutes.
1st experiment	CO_2	3·8	11·4	14·8	—	—
	O.M.	9·5	12·9	14·8	—	—
2nd experiment	CO_2	..	..	13·1	23·5	28·2
	O.M.	..	..	14·2	15·9	17·0
3rd experiment	CO_2	..	..	17·2	24·1	32·1
	O.M.	..	..	13·5	15·7	20·3

Here it is seen that the amount of organic matter becomes greater as the period of vitiation increases, but very much less slowly, so that the increase in the quantity of organic matter is by no means proportional to the time. It also increases less rapidly than the carbonic acid.

(8.) An atmosphere which has been entirely at rest for some time is found to contain less organic matter than it did previously. This is not necessarily entirely due to the settling down of the solid organic dust, but is probably due in part to oxidation.

The statement made in 'Nature' (vol. 33, 1886, p. 130), in an article on ventilation, to the effect that the organic matter in respired air increases *pari passu* with, and is therefore estimated by the amount of carbonic acid present, may be true when the average of a large number of determinations is taken, but is certainly very far from being true in individual cases. At any rate the amount of carbonic acid is no certain index of the quantity of organic matter present in an atmosphere (see above). That air in which respiration has gone on for some time gives invariably a higher result than outside air at or about the same time is all that can be confidently affirmed.

It should not be forgotten that the organic matter in air is most probably partly solid and partly gaseous. The solid obeying a different law than diffusion slowly settles down, whilst the gaseous part, unlike carbonic acid, is most likely an unstable compound or compounds, and readily undergoes oxidation. Experiments were made in regard to this, but they did not give decisive results.

November 18, 1886.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

The Right Hon. Lord Thurlow was admitted into the Society.

Professor Bonney, Sir James Cockle, Mr. Preece, Dr. Rae, and Mr. Stainton, having been nominated by the President, were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Method of Condensation in Calorimetry." By J. JOLY, B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Professor FITZGERALD, F.R.S. Received June 28, 1886.

(Abstract.)*

A substance at the initial temperature $t_1°$ of the atmosphere, suddenly immersed in a saturated vapour at the higher temperature $t_2°$, abstracts from it a quantity of heat equal to $WC(t_2° - t_1°)$, where C is the thermal capacity of the substance between the limits $t_1°$ and $t_2°$, and W its weight. There is then a weight w of the vapour precipitated, so that

$$WC(t_2° - t_1°) = w\lambda,$$

λ being the latent heat of the vapour.

By the observation of the weights and the temperatures, either c or λ may be the unknowns sought from the equation. The method is, in short, applicable to the determination of the specific heat of a substance or the latent heat of a vapour.

The paper contains an account of experiments illustrating the application of this method of condensation to the determination of the specific heats of substances. The condensation of steam is employed, its latent heat being accurately known, and its use affording

* This paper is printed in full at p. 352.

a considerable range of temperature. Two forms of the apparatus employed are described. The calorimeter consists essentially of a vessel of thin metal in which the substance is suspended by a fine wire, the wire issuing through an aperture in the top of the vessel and reaching up to one arm of a balance. The vessel is so constructed that steam can be passed through it from a small boiler, displacing the air around the substance. The substance rests on a light carrier of platinum wire provided with a little catchwater beneath to receive the drops of condensed water.

The manipulation involved is very simple. The substance being placed on the carrier which depends from the balance, is counterpoised. The calorimeter is then closed around it, the suspending wire passing freely through the aperture provided. This aperture is formed in an absorptive material (plaster of Paris), which ensures that it remains free of precipitated water. A thermometer reading to $\frac{1}{10}$ degree C. is inserted in the calorimeter, and allowed to remain with the substance for an interval sufficient to secure close equilibrium of temperature. The thermometer being read is removed, and the calorimeter suddenly placed in connexion with the boiler, which supplies a brisk current of steam and fills it immediately. It then remains to note the increment of weight when the substance has finally attained the temperature of the vapour. This stage is revealed in the persistent equilibrium of the balance. $t_2°$ is observed directly by a thermometer inserted in the boiler, or deduced by noting the height of the barometer and seeking the corresponding temperature of saturated steam from Regnault's tables. A correction is applied to the weight observed in experiment, necessitated by the difference of the weights of the displacement of the substance in air and in steam. In accurate experiments the value of λ is corrected according to Regnault's formula for its true value at $t_2°$.

The method is convenient for the reasons that it involves no preparations as in Bunsen's change of state method, no delicate thermometry, and the calorimeter being roomy permits of bodies of various shapes and bulks being dealt with. The apparatus, too, is of a simple and durable nature.

The experiments quoted in support of the method are (1) on the metallic elements, zinc, silver, lead, platinum, and aluminium. The results are in accord with those of Regnault, Bède, Mallett, &c. The degree of consistency between the experiments is greater than that attained in Regnault's researches. (2) On pure water sealed in thin glass bulbs. The results agree closely with the values deduced from Regnault's formula. (3) On mineral substances in various states of aggregation. It appears from these that the result is but little influenced by the extent of surface exposed to the steam.

The accuracy displayed by the method is explained on the probable

supposition that the substance is throughout the period of heating coated with a film of water adiathermanous and the external surface of which may be considered as appreciably that of the steam. The danger then of radiation error, that is, of steam condensing elsewhere than at the surface of the substance, is small. Condensation, in short, may be considered as taking place by abstraction of the energy of the molecule on impact with the water film.

II. "On the Specific Heats of Minerals." By J. Joly, B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Professor Fitzgerald, F.R.S. Received June 28, 1886.

A number of experiments were made on minerals by the method of condensation, using the form of gravimetric calorimeter described in the beginning of the paper on calorimetry (p. 353). The condensation of steam being in all cases employed, the values recorded are the mean specific heats between atmospheric temperatures approximating to 12° C., and steam temperature, about 100° C. More exactly, the values recorded are the mean calorific capacities for a rise of one degree between the limits t_1 and t_2, tabulated in each case. The specimens dealt with were chosen as good samples of the mineral free from visible impurities.* But before detailing the particulars of the experiments a few notes on the discriminative value and physical interest attached to this application of calorimetry may not be amiss.†

It seems probable that the neglect of the use of the specific heat constant is to be ascribed to the difficulties besetting its determination. Certainly if its determination was as easily effected as we effect the determination of the specific gravity of a body, there are on the other hand sufficient reasons to recommend its use as in general of more physical value and interest than the much used specific gravity. There are cases indeed where specific gravity, as it is possible to obtain it, is misleading, and where specific heat gives at once valuable information on the probable chemical nature of the substance. Such cases would arise with bodies of loose vesicular or hollow structure. No misleading variations need be introduced into the thermal constant by mere conditions of volume.

The method of condensation permits of the determination of this constant with very little experimental difficulty. On the whole the

* I have to thank Professor Sollas for the loan of useful specimens from the Museum of Trinity College.

† In November, 1883, I suggested this use of calorimetry to the Experimental Science Association, Trinity College.—"On the Determination of Minerals by their Specific Heats."

process is involved with no more liability to error than the process of weighing a body in air and in water. If, however, accuracy —greater than 1 per cent.—be desired, the operation takes considerably more time, as it is necessary, in securing t_1 accurately, to leave thermometer and specimen a sufficient time together in the calorimeter. The simplicity of the method is perhaps best inferred from the fact that of over 130 experiments on minerals there were but three spoiled—due to mishaps; and it will be seen that the repetition experiments reveal in no case appreciable discrepancy.

That this specific heat constant is very sensitive to the presence of impurities or variations in the chemical nature of the substance cannot be considered a disadvantage. It is supposable that this might lead occasionally to useful enquiry. This sensitiveness is shown in the case of tourmaline. Of three specimens the specific heats were—

Hemihedral, *black* crystal	= 0·2000
Striated, *black* crystal	= 0·2008
Hemihedral, *brown* crystal	= 0·2111
The same, a second experiment	= 0·2112

The variation from the black to the brown is over 5 per cent. Now tourmaline experiences considerable variations in the two constituents iron and magnesium. Approximately in percentage composition according to Dana's table of analyses—

	FeO.	Fe_2O_3.	MgO.
Black tourmaline contains	6	8	1
Brown tourmaline „	0	1	11

which in view of the high molecular heat of magnessia and the low molecular heats of the oxides of iron would account for the differences in the experiments. It will be seen later in the particulars of these experiments that the densities of these crystals also, in a less degree, reveal the difference in composition.

Again attention is suggestively drawn by the thermal constant to the chemical compositions of the first two of these three aerolites from the Museum of Trinity College.

(1.) Fell in Co. Limerick, September 10th, 1813	0·1787
(2.) Fell in Maryland, U.S.A., February 10th, 1825....	0·1785
(3.) Fell in Spain, July 8th, 1811	0·1856

The specific gravities, too, of the first couple, are found to be identical, thus: (1) =3·604; (2) =3·601; (3) =3·435. The experiments, in fact, evidently afford strong reasons for believing that the first two aerolites constitute a case of the strange similarity alluded to by Daubrée:—"Il y a des météorites éloignées au double point de

vue géographique et chronologique qui présentent parfois l'identité la plus complète, de telle sorte qu'il est impossible d'en distinguer les échantillons respectifs."* He adds a list of the few such occurrences known.

It is an important feature that the value of the thermal constant admits of being approximately, sometimes closely, calculated on an assumed chemical composition, and thus can be used in identification where comparative data do not exist or are not at hand. This fact rests on the experimental basis that the specific heat of the atom is preserved nearly constant through various atomic groupings. This applies indeed, only so far as the specific heats of the elements have been determined in the solid state; when the generalisation is applied to molecules containing elements whose specific heats as solids have never been directly determined it ceases to be accurate, and it appears at present as if in each kind of grouping a particular thermal capacity must be assigned to the atom. Approximate values of these thermal capacities have, however, been calculated by Kopp and others, and thus it is possible in all cases to pronounce for or against an hypothesis as to the nature of a substance, and this not alone in the case of simple salts, but when dealing with the silicates where many different kinds of atoms are present.

In the case of the bisilicate beryl, for example, the most probable formula is $5BeO,2Al_2O_3,H_2O,11SiO_2$.† Of these molecules the specific heat of BeO has alone not been directly determined. We take for the specific heat of Be 0·430,‡ and taking the atomic heat of solid oxygen as 4·5—the value it affords approximately in compounds having the constitution RO§—we assume its sp. h. in the molecule to be 0·281. On these assumptions the sp. h. of BeO would be 0·335. The atomic weight of Be is taken as 9·1. Taking advantage, then, of direct determinations as far as possible, the data are, sp. h. of BeO 0·335, of Al_2O_3 0·198,‖ of H_2O 0·501,¶ of SiO_2, 0·188.** From these, by an equation similar to Woestyn's—††

$$WS = n_1w_1s_1 + n_2w_2s_2 + \ldots,$$

where s_1, s_2, . . . are the specific heats of the several kinds of molecules as above; n_1, w_1, n_2, w_2, . . . the numbers and atomic weight of

* 'Géologie Expérimentale, Constitution des Météorites,' p. 507.

† From Penfield's result that the composition is $Be_5,Al_4,Si_{11}O_{34}$, 'Nature,' vol. 30, 1884, p. 378. The ratio is not quite bisilicate. Mr. Penfield thinks, however, that water should be included in the formula.

‡ 11° to 100°, Humpidge, 'Roy. Soc. Proc.,' vol. 39, 1886, p. 8.

§ Kopp.

‖ From my own results on corundum, agreeing with Regnault's.

¶ Person's result on ice, −20° to 0°. Landolt and Börnstein's 'Tabellen.'

** My own result on quartz, agreeing with Neumann's.

†† 'Annales de Chimie,' vol. 23, 1848, p. 295.

each kind of molecule present in the substance; W the molecular weight of the entire compound $= \Sigma wn$; s the required specific heat —the sp. h. of beryl is found to be 0·2140. The experimental results are

α. Transparent crystals	0·2066
β. Clouded crystals.....	0·2126

With the last variety the agreement is very close, but in either case the agreement is sufficient to render the calculation strong evidence had the case been one of hypothesis as to the nature of the substance.

I proceed to quote a case where the identification of a mineral was in this way left to the calorimeter.

The mineral had been assigned a place in a small collection as cryolite, on supposition. Its sp. h. was found to be 0·2558. This at once distinguished it from gypsum—sp. h. = 0·278—which it resembled in appearance. As I knew of no experiments on cryolite it was necessary to calculate the constant. The formula of cryolite—$3NaF + Al_2F_3$—includes the sp. hs. of Na, F, and Al. Of these F has not been directly determined. Its probable value was deduced from the sp. h. of fluorspar, which I had found to be 0·2118. Ascribing to the calcium in this compound the value 0·170 (Bunsen), the sp. h. of fluorine is deduced as 0·251, which agrees with Kopp's result. The other atoms in cryolite were assumed as follows: to the sodium was assigned Regnault's value 0·293; to the aluminium that obtained by myself, 0·223. On these assumptions the calculated sp. h. of cryolite is 0·2569. The experiment on the hypothetical cryolite affording 0·2558, the diagnosis was assured. A test of fusibility now confirmed the result, and subsequently an authentic specimen of cryolite gave 0·2538.

The calculation may also be effected on the percentage composition. It is a readier method but, it is remarkable, not so agreeable with the practical result. Thus in the case of cryolite, Al = 13·21, F = 53·56, Na = 32·65 in the mean analysis (Dana), and by the equation

$$S \times 100 = w_1 s_1 + w_2 s_2,$$

where w_1, w_2, . . . are the percentages, s_1, s_2, . . . the sp. hs. of the constituents, the result is S = 0·2591.

The specific heats of a few substances occasionally confounded are gathered here.

Apatite, opaque	0·1920
„ translucent.....	0·1829
Beryl, clouded	0·2127
„ transparent.....	0·2066

Orthoclase, transparent	0·1869
„ opaque	0·1890
Labradorite	0·1933
Oligoclase, transparent	0·2059
„ sub-translucent	0·1997
Albite	0·1983
Microcline	0·1905
Tourmaline, black	0·2004
„ brown	0·2111
Epidote, dark-green	0·1877
Hypersthene, dark-brown	0·1790
Amphibole, black	0·1963
Quartz	0·1881
Topaz	0·1997

Many of these substances may, of course, be also distinguished by their specific gravities. There is indeed, as might be expected, a connexion apparent between the two, the quantities tending to vary inversely, not only from one substance to another, but in the case of variations in specimens of the same species.

In its physical interest I think calorimetry deserves the attention of mineralogists. From results which came under notice in the course of the experiments on mineral crystals, I think it will be allowed that questions of interest are raised. These observations are in one direction only, and I have not had leisure to pursue them with the thoroughness they appear to me to deserve. Many questions suggest themselves on molecular freedom within the crystal which will need care and preparation to answer, and I now but give in outline an account of observations made, I may say, incidentally.

The crystallographic interest of the thermal capacity is based upon what is apparently an intimate connexion with the amount and kind of freedom possessed by the atoms of the solid. This is perhaps hardly unexpected. It is probable that calorific capacity, tending to be reciprocal to the mass of the atom, is more concerned with the intrinsic nature of the atom than with its surroundings. Still on any hypothesis of a vibratory molecule it is easy to conceive that if the surroundings are such as to hamper its freedom of motion the constraint will be revealed in its specific heat; that is, if we assume its increase of energy to be kinetic, as in increased amplitude of vibration. It might be expected then that in the crystal the atom would reveal thermally greater freedom than in the amorphous state. If, in fact crystalline arrangement be regarded as the result of harmony among molecular vibrations, we would expect for the crystal a specific heat approximating to that of the free molecule, and the more nearly approaching it the more perfectly harmony prevailed in the crystal.*

* There are not many experimental data for comparing the specific heat of the

It is here assumed that in some way similar to that in which the atom passes into the compound molecule without, in general, sensible change of thermal capacity, the molecule passes into the crystal with only insignificant further change in thermal capacity; the crystal being regarded in some sense as the chemical combination of the molecule. Thus we expect the specific heat of the crystal to be lower than that of an amorphous aggregation of its atoms under the constraint of discord. This agrees with experiment, for it may be stated as true generally that in the crystal the specific heat is lower than in the amorphous state, and that the more perfect and complete the state of crystallisation the lower the observed specific heat.

This, the first fact brought to my notice as experiments multiplied, seems to hold through the nicest indications of crystalline state as facial lustre, degree of translucency, as well as through the more marked indications of cleavage and form, and is probably quite in accord with the well-known fact that rolling, hammering, and such operations as tend to increase the density, diminish the specific heat of metals. The same would, doubtless, be found to hold in the case of the crystalline polarity conferred on metals by vibration.

I might illustrate the fact by a considerable number of cases where it happened that the specimens dealt with showed differences in crystalline state. I will only quote a few, referring to the tables for other cases. It is apparent in the case of *Barite*:—

Limpid crystal.....	0·1092
Sub-transparent crystal.....	0·1105
Opaque, lamellar.....	0·1116

In the case of *Gypsum*:—

Transparent.....	0·2726
Opaque, rough.....	0·2737

Sphalerite:—

Crystal, high lustre.....	0·1144
Crystalline, divergent.....	0·1155
„ massive.....	0·1162

Orthoclase:—

Limpid.....	0·1869
Opaque, well-formed crystals ..	0·1886
Massive, cleavable.....	0·1899

Fluorspar, cryolite, calcite, and quartz-opal are also examples. The case of opal is, however, uncertain, owing to probable presence of

molecule in crystalline freedom with the specific heat of the molecule in gaseous freedom. In the case of ice and steam, they are the same nearly; (Regnault) ice (−78° to 0°) = 0·474; steam (128° to 220°) = 0·480°.

water. I came on no exceptions. There is, indeed, a more conspicuous kind of variation shown in the specific heats of minerals not always determinable by external appearances, but this cannot, I think, be classed with the foregoing. It is, however, difficult at present to draw the line between them. But so far it seems highly probable that in slight differences of thermal capacity may be recognised a molecular restraint or discord or whatever molecular condition results in opacity, roughness, incompleteness, formlessness.

That other kind of variation to which I allude is of a different order of magnitude, and, I venture to suggest, points to causes of different order. It suggests the possibility that more than one molecular arrangement may obtain in bodies chemically and crystallographically, to all appearance, identical. For example, in hexagonal prisms of beryl two very different degrees of molecular freedom would appear to be possible. Certainly two very different thermal capacities obtain, unexplained by chemical differences or crystallographic form, so far as I can ascertain. The variation, too, seems in no way connected with the density of the substance, for this may be invariable from one specimen to another, while the difference of specific heat is remarkable. In many cases this difference of structure, if such it be, is revealed in the behaviour of the substance to light, in the opacity or transparency of the crystal. So marked is this that specific heats may be at once assigned with certainty to different crystals of known minerals on their opacity or transparency. In some cases again the substances present no differences whatever in appearance. The case of beryl stands thus:—

		Sp. h.	Sp. gr.
α	Transparent, blue, six small crystals....	0·20587	2·676
	„ blue crystal	0·20705	2·666
	„ greenish crystal	0·20636	2·660
β	Sub-translucent, green crystal	0·21257	2·706
	„ „ „	0·21236	2·706
	Opaque, dull-greenish crystal.....	0·21306	2·644

Between the α and β specimens above there is no difference apparent in crystallographic development save in the case of a couple of the small α crystals of the first experiment, which show pyramidal as well as the basal and prismatic faces common to all the specimens. Examination of the twenty analyses collected by Dana ('Mineralogy') suggests no explanation on chemical grounds. The percentages throughout vary in no adequate degree, ascribing to the molecules the thermal values recorded previously in arriving at a result by calculation agreeing with the β specific heat. I do not think, too, that results independent, as they constantly seem, of density can with probability be ascribed to differences in the amounts of the constituent

atoms. There is more probably some difference in constraint, of a structural kind, affecting the freedom of the molecules. Des Cloizeaux is of opinion that the optical examination of emerald, "and above all of beryl," points to a biaxial constitution.* In recently noting on these same clouded green β-beryls from our Dublin granite, I had occasion to remark on their optical heterogeneousness.† The extinction of basal sections is not uniform between crossed Nicols; pale grey cross-hatching is apparent, and as the mineral is rotated extinguishes capriciously over the field. In these patches the restoration of illumination is feeble and suggests imperfect coincidence of optic and crystallographic axes.

A still greater variation appears in the sp. h. of corundum from the clouded to the limpid variety, basing the sp. h. of the latter on Regnault's result on sapphire. It is remarkable, however, that the variation is in the opposite direction—the limpid variety exhibits the higher, the clouded the lower sp. h. Thus:—

α	Corundum	0·197.	R.
	Clouded crystal of corundum	0·198.	J.
β	Sapphire	0·217.	R.

This is a variation of 10 per cent. about. Now the analyses given in Dana's 'Mineralogy' of sapphire and corundum, although variable and pointing sometimes perhaps in the direction of the calorimetric result, certainly warrant no such difference, while on the other hand the sp. gr. of the two varieties are invariably opposed to the result being lower in the common variety than in the gem. It is perhaps remarkable that both these minerals, beryl and corundum, are hexagonal. A third case occurs in the case of calcite. I experimented on Iceland spar and on a clouded milk-white rhombohedron, and also on a specimen consisting of an aggregation of dihexagonal prisms. Adding the observations of others an α and β variety of calcite is suggested.

		Sp. h.	Sp. gr.	Observer.
α	Iceland spar	0·2036	2·713	J.
	Clouded rhombohedron	0·2044	2·702	J.
	Iceland spar	0·2046	—	N.
β	Translucent di-hexagonal prisms	0·2091	2·658	J.
	Iceland spar, two specimens	0·2086	—	R.

In addition there is an observation of Kopp's 0·206; it is highly probable, however, from a consideration of Kopp's general results, his method of working, and, indeed, his own acknowledgement, that this is too low. It is likely he was dealing with β-calcite. It is to be remembered that Iceland spar is chemically a very pure form of the mineral.

* 'Minéralogie,' vol. 1, p. 366.

† [1885] 'Roy. Dublin Soc. Proc.,' vol. 5, p. 49.

The case of aragonite is unexpected. Its specific heat has been obtained by Regnault, Kopp, Neumann, and myself. The experiments fall into α- and β-groups, having values identical with those of calcite, and the remarkable fact appears that although the minerals differ in crystalline symmetry and specific gravity, the evidence is in favour of ascribing the same thermal capacities to both. The experiments are—

		Sp. h.	Sp. gr.	Observer.
α	Two transparent crystals (Bohemia)	0·2040	2·955	J.
	„ „ „ (Auvergne)	0·2039	2·955	J.
	Aragonite	0·2030	—	K.
β	„	0·2085	—	R.

Neumann's result, 0·2018, probably comes under the α variety. A coincidence of error adequate to account for this nearly unanimous agreement between the two forms of calcite is improbable. The inference would appear to be that in both forms the molecules possess nearly equal freedom. From my own results on transparent crystals of both forms the molecular restraint seems perhaps greater in aragonite. This is probable, too, from the phenomena attending the heating of aragonite. The crystals spontaneously break and develope cracks even at the low temperature of the steam ; a phenomenon which, be it observed, probably in some slight degree falsifies the results.

The sp. h. of a specimen of massive rough barites from Glendalough, co. Wicklow, is sufficiently remarkable to justify notice. In this case the nearly pure nature of the specimen was placed beyond doubt by analysis. The sp. h. obtained was—

	Sp. h.	Sp. gr.
Rough crystalline barites	0·10316	4·172

my other observations on this mineral ranging from 0·1092 to 0·1117. Neumann records 0·1088 ; Regnault, 0·1128. As the specimen was unaffected by acids, the presence of $PbSO_4$ was suggested as the only likely admixture competent to lower the sp. h. On this account I analysed the substance. The result is—

$BaSO_4$	99·1
MnO_2	0·3
$CuFeS_2$	0·2
Pb	trace
Ca	trace
Fe_2O_3	0·6
	100·2

The specific gravity, it will be seen, is also abnormal for barites (4·3—4·7, Dana). The sp. gr. of the other specimens examined by

me approximate to 4·4. It is certain that the small quantities of impurities present, acting merely by their atomic heats, would not suffice appreciably to alter the sp. h., and would rather tend to elevate it.

To these cases may, perhaps, be added those of apatite and oligoclase. I examined but two specimens of each.

Apatite.

	Sp. h.	Sp. gr.
Translucent hexagonal prism, high lustre, brittle	0·1826	3·166
Opaque, dull compact crystal	0·1920	3·089

Oligoclase.

	Sp. h.	Sp. gr.
Sub-translucent, milk-white (Ytterby)	0·1997	2·621
Transparent crystal	0·2059	2·605

It remains to notice the cases of three chemically and crystallographically related minerals where the variations obtaining in each case would appear to stand in a numerical relation from one substance to another. The minerals are iron pyrites (isometric disulphide of iron); galenite (isometric sulphide of lead); sphalerite (isometric sulphide of zinc). The orthorhombic disulphide of iron, marcasite, seems also to fall into the relationship.

The specific heat of pyrites is, according to Regnault, 0·13009. Operating on a large bright cube, I obtained the same number exactly, 0·13009. Neumann's result is 0·127, Kopp's 0·126. The specific heat of galena is given differently by Regnault, Kopp, and Neumann. The value given by Kopp, 0·0490, closely agrees with an observation of my own, 0·0492. I also obtained 0·0504 and 0·0522 on well crystallised specimens. The last number is probably a little excessive, as there was just a trace of a white substance—the carbonate—present. There are found finally for pyrites and galena the numbers—

Pyrites.

N.....	0·127	.....	R..... 0·130
K.....	0·126	.....	J..... 0·130

Galena.

K.... 0·0490	R.... 0·0508	J.... 0·0522	N.... 0·053
J.... 0·0492	J.... 0·0504		

Taking the means of the first two vertical colums in each case, the proportion holds very closely

Pyrites. Galena.

126·5 : 130 :: 491 : 506.

The experiments are so close that any of the recorded values are fairly in the proportion, and some evidently better than the means; thus if my own results and Neumann's be alone considered, it will be found that the ratio is almost absolute. As it stands, on the mean values, the products are as 638 to 640.

If Neumann's result for marcasite, 0·133, be included, we find the more extended proportion—

Pyrites.			Galena.	
126·5	: 130 :	133 ::	491 :	506 : 522.

But in this the last number is a little excessive, which, as observed, is probably due to impurity.

Two distinct values, very different, are on record for *sphalerite*:—

N.....	0·114	.....	R.....	0·123
J.....	0·115	.....	K.....	0·120

and finally the proportion with the extreme values of pyrites and galena obtains.

Sphalerite.		Pyrites.		Galena.
114·5	: 121·5 ::	126·5	: 133 ::	491 : 522.

If this proportion be examined by products of consequent and antecedent of first term with sum of antecedents and consequents, the products obtained are as 88,938 to 88,909. One value only, it will be seen, is omitted from the ratios, that of Neumann, for galena. It appears to lie outside the proportion, and may either be erroneous or simply indicative of corresponding terms not yet found for the other sulphides.

In conclusion, I would ask in reference to the foregoing remarks, if in the present state of our knowledge there is anything *à priori* improbable in different molecular arrangements or orientations obtaining under the same crystalline form and affecting to definite extents the thermal freedom of the molecule. Such differences of arrangement, hardly detectable perhaps by any other means of investigation, might exist unnoticed except when, as possibly occurs in the case of marcasite, prevailing to a degree competent to determine a different symmetry for the aggregate.

Regarding the experiments which follow, I have only now to observe on the precautions observed in effecting them.

The thermometers, certified at Kew, read directly to 0·1° C.; the place of hundredths being obtained by estimation.

The balance used was trustworthy to 0·0005 gram. To secure t_1 accurately indicating the temperature of the substance, thermometer and specimen were left together in the calorimeter—starting at air temperature—never less than one and a half hour; large specimens,

two to three hours; and the largest were reserved for leaving in all night.

Specimens presenting at all a porous or loose surface were carefully dried, often for many days, in an oven, and subsequently preserved over calcium chloride. Porous or loose substances, not of special value, were, for greater surety in drying, broken into small bits—as in the case of the few rock specimens investigated. To preserve these dry in the calorimeter, while acquiring air temperature, calcium chloride was in some cases placed in the little chamber projecting from the lower part of the sphere. If counterpoised immediately on being placed in the calorimeter absorption of moisture may evidently be detected, weighed, and its thermal capacity allowed for. This plan was on a couple of occasions resorted to. In any cases observed, however, entire neglect of this absorption would have produced but small error.

Supposing errors of the same sign to accumulate through an experiment, I do not think they can have in any case falsified the value of the sp. h. above 0·4 per cent. This seems probable, too, from the agreement observed on repetition.

Allowance has in all cases been made for the variation of λ with t_2, according to Appendix of last paper. The effect of the low specific gravity of steam on the apparent weight of condensation has been allowed for, as $-\frac{W}{\text{Sp. gr.}} \times 0{\cdot}62$ gram. With these deductions and allowance for the precipitation on the carrier and bucket according to range of temperature obtaining, the value of w is deduced as recorded in the tables.

The specific gravities recorded have been reduced to water at 4° C.

Specific Heats of some Minerals.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Galenite, PbS. Isometric :—						
Four well-formed crystals—cubo-octahedral	62·964	13·27	99·50	0·5105	0·05406	7·323
Close grained fragments	87·840	13·27	99·60	0·6955	0·04921	7·562
Very cleavable fragments	56·967	10·50	99·90	0·496	0·05224	7·365
Another experiment	56·951	10·98	99·76	0·493	0·05232	,,
,, ,,	56·951	11·77	99·70	0·487	0·05219	,,
Other observers..... R.=0·05086; N.=0·053; K.=0·0490.						
Sphalerite, ZnS. Isometric :—						
Cleavable, high resinous lustre, dark brown	45·765	11·80	100·18	0·863	0·11445	4·082
Fibrous, divergent, dark brown	80·520	11·40	100·30	1·610	0·11553	4·039
Compact, crystalline, dark brown	31·893	9·90	99·90	0·622	0·11625	4·020
Other observers..... R.=0·1230; N.=0·1145; K.=0·120.						
Pyrite, FeS_2. Isometric :—						
Large bright cube	55·890	12·23	99·70	1·1785	0·13009	4·938
Cubes stained with limonite	73·309	12·40	99·40	1·597	0·13443	4·882
Cubes and pyritohedrons, bright	57·671	12·30	99·70	1·2265	0·13059	4·808
Two interpenetrated cubes, bright, pale	46·710	10·50	99·90	1·016	0·13111	4·854
Other observers..... R.=0·13009; N.=0·1275; K.=0·126.						
Chalcopyrite, $CuFeS_2$. Dimetric :—						
Crystalline fragment, bright, iridescent	48·687	11·40	100·00	1·0225	0·12717	4·143
Another experiment	48·645	12·55	99·90	1·0065	0·12708	,,

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Fluorite, CaF. Isometric :—						
Clear green cube	32·957	10·40	100·00	1·166	0·21180	3·172
Crystalline, translucent, green	31·275	9·75	100·10	1·117	0·21264	3·171
Other observers..... R.=0·2149; N.=0·2082; K.=0·209.						
Cryolite, $3NaF + Al_2F_3$. Trimetric (?) :—						
White, subtransparent	58·438	13·00	100·25	2·480	0·25385	2·963
„ translucent	43·060	11·50	99·70	1·8105	0·25583	2·961
Other observers..... K.=0·238.						
Corundum, Al_2O_3. Hexagonal :—						
Hexagonal prism, clouded, pale blue	102·820	12·56	99·90	3·314	0·19800	3·963
Another experiment	102·820	12·62	99·80	3·3135	0·19820	„
Other observers..... R.=0·19762, and sapphire 0·21732.						
Hematite, F_2O_3, Rhombohedral :—						
Botryoidal, divergent	61·175	13·50	99·80	1·6495	0·16766	4·644
„ „	67·604	7·80	100·40	1·976	0·16893	4·923
Other observers..... Specular iron, R.=0·1670; N.=0·1692; K.=0·154.						
Limonite, $F_2O_3 + 3H_2O$:—						
Divergent, brown, silky	58·990	13·27	99·78	2·107	0·22632	4·037
Pseudomorphous after pyrites, brown	26·492	13·63	99·65	0·961	0·22155	3·886

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Quartz, SiO_2. Rhombohedral :—						
Ten small crystals, limpid, R, *i*, −1	32·921	11·95	99·75	1·0125	0·18800	2·649
Smoky crystal, trapezohedral (left, tetartohedral) with R, *i*, −1	29·743	13·10	99·60	0·9025	0·18828	2·649
Clear limpid, R, *i*, −1	17·572	11·83	99·30	0·539	0·18827	2·652
Clear, R, *i*, −1	14·686	16·50	100·22	0·4305	0·18780	2·649
Other observers..... R.=0·1913 ; N.=0·1883 ; K.=0·186.						
Opal, SiO_2. Amorphous :—						
Hyalite, limpid, spheroid aggregations	28·468	13·25	99·60	0·931	0·20330	2·168
White, nearly opaque	22·456	8·20	99·90	0·913	0·23785	2·106
Another experiment	22·437	13·20	99·40	0·855	0·23733	„
Other observers..... K.=0·197 (Hyalite) ; K.=0·208 (noble opal).						
Hypersthene, $(MgO,FeO)SiO_2$. Trimetric :—						
Crystalline, massive, dark brown	69·398	13·55	100·08	2·0045	0·17900	3·819
Another experiment	69·336	13·25	99·80	2·001	0·17891	„
Amphibole, Bisilicate of Protox. and Sesquiox. bases. Monoclinic :—						
Five black crystals, O.I. $\bar{n}$, −1	57·192	12·00	99·72	1·8355	0·19634	3·183
Two largest of the five	45·133	12·94	100·30	1·4435	0·19635	„
Three largest of the five	51·309	12·90	100·30	1·6405	0·19619	„
Probably aluminous magnesia-lime-iron hornblende.						

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Beryl, $3BeO + Al_2O_3 + 6SiO_2$. Hexagonal :—						
Six small crystals of clear blue aquamarine	26·050	12·47	99·10	0·865	0·20587	2·676
Transparent blue aquamarine	13·966	11·72	99·91	0·475	0·20707	2·666
Another experiment	13·938	9·75	100·00	0·4885	0·20703	„
Transparent pale green	47·336	12·40	99·80	1·591	0·20636	2·660
Clouded green, well crystallised, high lustre	37·495	6·80	100·10	1·371	0·21257	2·706
Clouded green prism (Glencullen, Co. Dublin)	49·502	11·90	99·45	1·7145	0·21236	2·708
Clouded greenish-white, radiating (Glencullen)	13·016	16·69	100·07	0·431	0·21306	2·644
Garnet, $\dot{R}_6\ddot{\dot{R}}_2\ddot{S}i_6$. Isometric :—						
Two dark red crystals	50·940	10·65	99·90	1·5095	0·17813	3·978
Another experiment	50·940	12·65	99·90	1·474	0·17792	„
Dark red crystal	39·094	12·30	99·90	1·145	0·17934	3·757
Another experiment	39·094	12·60	99·90	1·141	0·17929	„
Principal bases probably Al_2O_3 and FeO.						
Vesuvianite, $(\frac{2}{3}\dot{C}a_3 + \frac{2}{5}\ddot{\ddot{A}l})_2\ddot{S}i_3$. Dimetric :—						
Two translucent dark green crystals, high lustre	24·879	12·53	99·78	0·790	0·19529	3·353
Another experiment	24·879	12·00	100·30	0·797	0·19457	„
Epidote, $\dot{C}a_6(\ddot{\ddot{A}l},\ddot{\ddot{F}e})\ddot{S}i_9$. Monoclinic :—						
Two crystals, fine lustre, black-green	21·300	13·50	99·45	0·639	0·18740	3·430
Another experiment	21·300	13·45	100·30	0·649	0·18792	„

Specific Heats of some Minerals—*continued.*

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Biotite, Magnesia-iron Mica :—						
Black, opaque flakes (Perth, Canada)	20·291	12·63	100·30	0·685	0·20651	2·841
Another sample of same	17·809	12·70	99·95	0·5935	0·20491	„
Muscovite, Potash Mica :—						
Silvery-white crystals (Beleek, Co. Fermanagh)	23·735	13·80	99·75	0·778	0·20519	2·725
Another sample of same in flakes	20·541	12·40	99·90	0·6875	0·20466	„
Lepidolite, Potash, Lithia Mica :—						
Massive, scaly-granular, fine lilac	42·726	12·20	100·20	1·4705	0·20978	2·833
Another sample of same	48·823	12·38	100·20	1·6760	0·20967	„
Wernerite, $\dot{C}a_3\ddot{\ddot{A}}l_2\ddot{S}i_6$. Dimetric :—						
Two interpenetrating prisms, opaque, rough, grey	34·156	10·55	100·00	1·141	0·20036	2·590
Another experiment	34·156	12·10	100·30	1·125	0·20029	„
Leucite, $KO,Al_2O_3,4SiO_2$. Isometric :—						
Two trapezohedral crystals, grey	15·937	12·00	99·95	0·4995	0·19119	2·454
Another experiment	15·937	12·84	99·90	0·496	0·19178	„

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Labradorite, Lime-soda felspar. Triclinic :—						
Chatoyant polished specimen	44·155	13·50	99·72	1·3705	0·19320	2·691
Another experiment	44·155	12·29	100·05	1·3975	0·19348	,,
Rough, chatoyant (Labrador)	39·790	12·25	100·20	1·2625	0·19351	2·693
A similar specimen ,,	43·946	12·60	100·18	1·386	0·19317	2·692
Oligoclase, Soda-lime felspar. Triclinic :—						
Subtransparent crystal	39·534	12·00	99·45	1·3255	0·20580	2·605
Another experiment	39·534	12·20	100·05	1·3335	0·20600	,,
Milk-white, subtranslucent (Ytterby)	48·074	12·60	99·10	1·5465	0·19974	2·621
Another sample of same	41·829	11·60	99·40	1·366	0·19967	,,
Albite, Soda felspar. Triclinic :—						
Group of limpid twinned crystals, composition on *it* (Tyrol)	23·991	12·50	100·20	0·8395	0·19846	2·630
Another experiment	23·979	12·63	100·18	0·7765	0·19840	,,
,, ,,	23·978	12·71	100·19	0·7753	0·19828	,,
Other observers..... K.=0·190.						
Orthoclase, Potash felspar. Monoclinic :—						
Transparent adularia	69·893	10·88	100·20	2·175	0·18688	2·561
Another experiment	69·887	13·20	99·76	2·108	0·18702	,,
Green-blue amazon stone, cleavable	66·632	13·10	99·90	2·0155	0·18696	2·565
Two crystals, a Carlsbad and a Baveno twin, opaque white	23·359	14·37	99·35	0·6975	0·18865	2·445
White, nearly opaque, cleavable (Co. Dublin)	27·490	11·03	99·80	0·8585	0·18877	2·551
Red, very cleavable, subtranslucent (Castlecaldwell)	48·279	11·80	99·50	1·502	0·18992	2·546
Other observers..... N.=0·1861 (adularia); 0·1911 (orthoclase); K.=0·183 (orthoclase).						

Specific Heats of some Minerals—*continued.*

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Microcline, Potash felspar. Triclinic :—						
Well-marked pink, cleavable (Glencullen, Co. Dublin)	30·863	13·45	99·10	0·931	0·18916	2·471
Another specimen from same locality	35·519	14·84	99·35	1·0745	0·19219	2·471
Large crystal, pink (Glenmalure, Co. Wicklow)	71·588	12·90	99·95	2·242	0·19302	2·531
Fawn-coloured, cleavable (Arendal)	33·124	12·37	100·19	1·031	0·19915	2·565
Another sample of same	29·052	12·50	100·20	0·9065	0·19084	2·559
Tourmaline, Subsilicate of MgO,BO_3,Al_2O_3,Fe_2O_3. Rhombohedral :—						
Hemihedral crystal, fine black lustre	30·588	11·78	99·50	1·0025	0·20005	3·139
Striated crystal „ „	24·919	9·08	100·10	0·846	0·20082	3·104
Hemihedral brown crystals, translucent, little Muscovite present	24·747	12·90	99·50	0·8425	0·21114	3·019
A second experiment	24·747	12·57	100·00	0·842	0·21126	„
The black is probably iron tourmaline; the brown magnesian.						
Topaz, $Al_2O_3\{\frac{1}{2}SiO_2+\frac{1}{2}SiF_2\}$. Trimetric :—						
Limpid fragment	37·571	10·60	99·70	1·2445	0·19989	3·568
A second experiment	37·571	15·40	99·80	1·179	0·19965	„
Colourless, hemihedral	18·852	11·38	99·75	0·621	0·19953	3·541
Prehnite, $\dot{C}a_2\ddot{A}l,\ddot{S}i_3+\dot{H}$. Trimetric :—						
Globular crystalline surface, translucent, green (Dumbartonshire)	39·105	12·12	99·45	1·276	0·20057	2·908
Another experiment	39·096	11·90	100·00	1·2845	0·20008	„

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Natrolite, $Na_2Al_2O_{10}Si_3 + 2H_2O$. Trimetric :—						
Radiating group of white translucent crystals (Auvergne)	13·660	11·70	99·90	0·532	0·23689	2·244
Another similar specimen (Auvergne)	24·762	12·67	99·90	0·959	0·23819	2·243
Stilbite, $CaAl_2O_{16}Si_6 + 6H_2O$. Trimetric :—						
Pearl-white radiating (Iceland)	28·583	11·25	99·40	1·230	0·26207	2·158
Dried at 100° (about) for some days before experiment.						
Talc, $6MgO,5SiO_2 + 2H_2O$. Trimetric :—						
Pale green soapstone	47·445	12·32	100·25	1·6855	0·21671	2·769
From same hand-specimen	44·631	13·30	100·20	1·568	0·21686	„
Serpentine, $3MgO,2SiO_2 + 2H_2O$. Trimetric (?) :—						
Green, translucent (Galway)	31·014	11·20	99·80	1·295	0·25288	2·549
Apatite, $3CaOPO_5 + \frac{1}{3}Ca(ClF)$. Hexagonal :—						
Green-brown prism, high lustre, subtranslucent, brittle	76·287	11·20	99·90	2·3035	0·18263	3·166
Another experiment	76·290	12·08	100·30	2·300	0·18327	„
Opaque pink-white prism, dull surface (Krageröe)	22·870	12·45	99·73	0·714	0·19196	3·089
A second experiment	22·870	12·10	100·10	0·721	0·19217	„

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Barite, $BaSO_4$. Trimetric:—						
Limpid crystal. O.I.$\breve{a}$ and brachydome; striæ on O	76·109	9·65	100·30	1·405	0·10923	4·475
Same in small pieces	65·143	9·60	99·80	1·195	0·10910	„
Yellow crystal, subtransparent	20·717	12·83	99·23	0·3685	0·11054	4·474
Opaque white, lamellar	48·103	12·60	99·20	0·8665	0·11169	4·324
„ „ rough*	49·863	9·80	100·40	0·869	0·10316	4·172
Other observers..... R.=0·1128; N.=0·1088; K.=0·108.						
Gypsum, $CaSO_4+2H_2O$. Monoclinic:—						
Transparent crystal of selenite	21·048	10·00	100·10	0·965	0·27264	2·311
Four transparent crystals of selenite	13·204	7·24	100·40	0·623	0·27164	2·313
Aggregate of acicular crystals	30·150	9·20	100·40	1·393	0·27167	2·399
Rough gypsum, white	31·547	9·98	100·40	1·456	0·27374	2·310
Other observers..... N.=0·2728; K.=0·259.						
Calcite, $CaCO_3$. Rhombohedral:—						
Limpid Iceland spar	33·904	10·00	99·76	1·156	0·20383	2·713
Same crystal in fragments	33·059	13·34	99·50	1·0795	0·20345	„
Three slender hexagonal prisms, limpid	10·044	13·04	99·60	0·3295	0·20340	2·710
Translucent white rhombohedron	35·815	16·75	99·93	1·135	0·20439	2·702
Group of tapering prisms, some dihexagonal, translucent	31·524	11·90	99·50	1·0755	0·20908	2·658
Chalk, white (Newhaven) well dried	22·454	12·30	99·80	0·7475	0·20415	—
Another specimen of same	21·737	13·77	99·35	0·7065	0·20390	—
This chalk dissolves in HCl with very little residue. Heat of humectation may lower result. Other observers..... R.=0·2086 (Iceland spar); N.=0·2046; K.=0·206.						

* For analysis, see *ante*, p. 255.

Specific Heats of some Minerals—*continued*.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Aragonite, $CaCO_3$. Trimetric :—						
Two transparent prisms, yellow (Bohemia)	24·331	12·45	99·65	0·8065	0·20401	2·955
„ „ „ colourless (Auvergne)	28·161	11·60	99·00	0·9345	0·20393	2·955
Other observers..... R.=0·2085 ; N.=0·2018 ; K.=0·203.						
Witherite, $BaCO_3$. Trimetric :—						
Translucent crystal, white	41·407	13·68	99·25	0·716	0·10850	4·288
„ „ „	64·172	12·90	99·75	1·130	0·10881	4·284
Other observers..... R.=0·1104 ; N.=0·1078.						
Malachite, $CuCO_3 + CuOH_2O$. Monoclinic :—						
Botryoidal, divergent, banded, compact	37·438	10·20	99·40	1·095	0·17653	3·801
A similar specimen	49·265	11·60	99·80	1·430	0·17659	3·831

On some Rocks.

	W.	t_1.	t_2.	w.	Sp. h.	Sp. gr.
Granite; quartz felspar mica :—						
Fine-grained Aberdeen granite, contains a black mica and pink felspar	31·253	10·98	99·85	0·998	0·18921	2·625
Coarse-grained, Ballyknockin (Co. Wexford), white felspar, silvery mica	42·105	13·50	99·70	1·312	0·19404	2·605
Medium grain, Killiney (Co. Dublin), white felspar, silvery mica	41·200	13·34	99·40	1·2725	0·19269	2·645
Owing to the close agreement between the sp. heats of quartz and orthoclase (0·188), orthoclase granites will not probably vary much in sp. heat.						
Basalt :—						
Fine-grained, black (Giant's Causeway)	24·400	10·17	99·85	0·814	0·19961	2·883
Hornblende Diorite :—						
Coarse, fibrous, actinolite trap, dark green (Glendalough, Co. Wicklow)	40·265	12·25	99·90	1·409	0·21135	2·996
Slate :—						
Fine green slate (Bray Head, Co. Dublin)	32·874	10·54	99·90	1·133	0·20695	2·722

On three *Aerolites*. Group—Sporadosiderites; sub-group—Oligosiderites; (Daubrée).

	W.	t_1.	t_2.	*w*.	Sp. h.	Sp. gr.
Fell at Adare, Co. Limerick, Sept. 10, 1813	67·095	12·52	100·18	1·9595	0·17875	3·604
,, Nanjemoy, Maryland, U.S.A., Feb. 10, 1825	38·865	12·25	100·18	1·1375	0·17854	3·601
,, Berlanguillas, Spain, July 8, 1811.....	21·664	12·67	100·18	0·656	0·18566	3·435

The three are similar in appearance, rough, grey, with small grains of nickeliferous iron and magnetic pyrites. Analysis of the first (Apjohn): Iron and nickel = 23·07, magnetic pyrites = 4·38, chrome-iron = 3·34, earthy matrix = 68·47, alkalies and loss = 0·74.

Alphabetical List of the Specific Heats of some Minerals.

Temperature limits about 12° C. to 100° C.

	Sp. h.
Aerolite, *a*.	0·1787
„ *b*.	0·1785
„ *c*.	0·1856
Albite, transparent crystals	0·1983
Amphibole, black crystals	0·1963
Apatite, translucent, green	0·1829
„ opaque white-pink	0·1920
Aragonite, transparent crystals	0·2036
Barite, transparent crystals	0·1096
„ opaque lamellar	0·1117
„ opaque, massive	0·1032
Basalt, fine black	0·1996
Beryl, transparent crystals	0·2066
„ subtranslucent	0·2127
Biotite, black	0·2057
Calcite, Iceland spar	0·2036
„ translucent rhombohedron	0·2044
„ hexagonal prisms	0·2034
„ „ „	0·2091
„ chalk	0·2040
Chalcopyrite, bright, iridescent	0·1271
Corundum, clouded	0·1981
Cryolite, translucent	0·2548
Epidote, crystals, dark green	0·1877
Fluorite, transparent crystal	0·2118
„ translucent	0·2126
Galenite, crystals	0·0505
„ crystalline	0·0492
„ „	0·0522
Garnet, red crystals	0·1780
„ a red crystal	0·1793
Granite, Aberdeen	0·1892
„ Wexford	0·1940
„ Killiney	0·1927
Gypsum, rough, white	0·2737
„ acicular	0·2717
„ selenite	0·2721
Hematite, botryoidal, divergent	0·1683
Hornblende, trap-rock, fibrous, green	0·2113
Hypersthene, massive, crystalline	0·1790
Labradorite, grey, chatoyant.	0·1933
Lepidolite, massive, crystalline	0·2097
Leucite, crystals	0·1912
Limonite, crystalline, massive	0·2215
„ fibrous, divergent	0·2263
Malachite, botryoidal	0·1766
Microcline, cross-hatched	0·1915
„ fawn-coloured	0·1905
Muscovite, silvery	0·2049
Natrolite, translucent crystals	0·2375
Oligoclase, subtransparent	0·2059
„ white, subtranslucent	0·1997
Orthoclase, transparent adularia	0·1869
„ subtranslucent, opaque	0·1890
Phrenite, botryoidal, green	0·2003
Pyrites, cubes	0·1306
Quartz, limpid	0·1881
„ transparent, hyalite	0·2033
„ white opal	0·2375
Serpentine, green	0·2529
Slate, fine green	0·2069
Sphalerite, well crystallised	0·1144
„ crystalline	0·1159
Stilbite, white radiating (dry)	0·2621
Talc, greenish-white soapstone	0·2168
Topaz, transparent, colourless	0·1997
Tourmaline, black crystals	0·2004
„ brown crystals	0·2111
Vesuvianite, green crystals	0·1949
Wernerite, opaque crystals	0·2003
Witherite, translucent	0·1086

III. "Note on a paper entitled 'On a New Form of Stereoscope' ('Roy. Soc. Proc.,' vol. 40, p. 317)." By A. Stroh. Communicated by Lord Rayleigh. Received September 28, 1886.

Since the reading of my paper on "A New Form of Stereoscope" ('Roy. Soc. Proc.,' vol. 40, p. 317) my attention has been called to a paper read by Mr. Howard Grubb, F.R.A.S., at the Royal Dublin Society, 20th January, 1879.

I find that this paper describes the essential points of the apparatus, and I therefore desire to acknowledge the prior publication by Mr. Grubb.

IV. "On the Intensity of Light reflected from certain Surfaces at nearly Perpendicular Incidence." By LORD RAYLEIGH, M.A., D.C.L., Sec. R.S. Received October 6, 1886.

[PLATE 3.]

In the present communication I propose to give an account of a photometric arrangement presenting some novel features, and of some results found by means of it for the reflecting power of glass and silver surfaces. My attention was drawn to the subject by an able paper of Professor Rood,* who, in giving some results of a photometric method, comments upon the lack of attention bestowed by experimentalists upon the verification, or otherwise, of Fresnel's formulæ for the reflection of light at the bounding surfaces of transparent media. It is true that polarimetric observations have been made of the *ratio* of the intensities with which the two polarised components are reflected; but even if we suppose (as is hardly the case) that these measurements are altogether confirmatory of Fresnel's formulæ, the question remains open as to whether the actual intensity of each component is adequately represented. This doubt would be set at rest, were it shown that Young's formulæ for perpendicular incidence (to which Fresnel's reduce), viz., $\left(\frac{\mu-1}{\mu+1}\right)^2$, agrees with experiment.

Professor Rood's observations relate to the effect of a plate of glass when interposed in the course of the light. He measures, in fact, the transmission of light by the plate, and not directly the reflection. No one is in a better position than myself for appreciating the advantages of this course from the point of view of experiment. In the first place, the incidence can easily be made strictly perpendicular, in which case no question arises of a separate treatment of the two polarised components of ordinary light. And, what is much more important, the interposition of the plate leaves the course of the light unchanged, and thus allows the alteration of intensity to be determined in an accurate manner with the simplest arrangements.

On the other hand, the measurement of the transmitted, instead of the reflected light, is open to grave objection on more than one ground. It may be doubted whether the influence of *absorption* is altogether negligible, even when the thickness of the plate is as small as that mentioned by Professor Rood, viz., 1·67 mm. But the feature which strikes me most unfavourably is the necessary magnification of error, when we deduce the proportion of light reflected

* 'Amer. Journ. Sci.,' vol. 49, 1870 (March); vol. 50, 1870 (July).

from the observed loss of light transmitted. The transmitted light is about 91 per cent.; and thus an error arising from the neglect of absorption, or from imperfect matches, amounting, say, to 1 per cent., leads to a relative error of more than 10 per cent. in the estimated reflection. The importance of this consideration may be illustrated by Professor Rood's actual results. In the first case recorded by him the observed transmission was 91·440 as against the theoretical 91·736. The difference 0·296 is indeed very small reckoned upon the transmitted light; but if we translate the results into terms of the reflected light, they present a different appearance. On the supposition that the whole loss in the transmitted light is due to reflection, we get for the intensity of the reflected light 8·560, which is to be compared with the theoretical 8·264. The difference 0·296 is now some $3\frac{1}{2}$ per cent., and is thus by no means insignificant. In the other case given by Professor Rood the discrepancy is greater still, amounting to 7 per cent. It may be remarked that in both cases the amount of the reflection appears to be in excess of that given by Young's formula. But the cause may lie in the assumption that the whole failure of transmission is due to reflection. And whatever the explanation may be, we can hardly agree with Professor Rood when he concludes that these experiments show "that the reflecting power of glass with the above index of refraction, *conforms in the closest manner to the predictions of theory*."

In the hope of being able to deal directly with the reflected light, I made a great many trials of various devices during the spring of 1885, but without finding anything satisfactory. Indeed, at one time, I had almost come round to the opinion that the difficulties of measuring the reflected light were so great that Professor Rood had shown a wise discretion in declining to face them, and that after all the best results would perhaps be reached through measurements of the transmitted light checked by the use of plates of different thicknesses so as to eliminate absorption.* If, indeed, we give up the perpendicular incidence, the objection founded upon the relatively small quantity of light reflected may be met; for at an incidence of 70°, about half the light (polarised in the plane of incidence) is reflected. In such an experiment it would of course be necessary to determine accurately the angle of incidence.

The difficulties referred to have their origin in the necessary alteration in the course of the light by the act of reflection. The direct and reflected light cannot be interchanged in any simple manner, and the shift necessary to bring about the substitution may easily lead to systematic error. In the apparatus (presently to be described) to which I was finally led, the difficulty seems to be fairly

* I was not aware until lately that Sir John Conroy was at work in this direction.

overcome so far as regards the accuracy of the results, but at the cost of several tiresome adjustments, impeding the ready trial and interchange of various reflectors.

My apparatus differs in several respects from that generally used for photometric purposes. Before describing it in detail, it may be worth while to indicate some of the considerations which led me to design it.

The photometers in ordinary use may be said to depend upon the principle of diffusion. If the illuminating candle, or lamp, be drawn back from the screen to double the original distance, the brightness of the screen as perceived by the eye is supposed to be quartered. This implies that (within certain limits) the brightness of the screen is independent of the apparent magnitude of the source of light (the total radiation being given), or that the light diffused by the screen in a particular direction (towards the eye) is independent of the direction of incidence. Reciprocally, the light incident in a definite direction is supposed to be diffused through a considerable angle with some approach to uniformity. There is no doubt that with proper arrangements this condition may be satisfied with sufficient accuracy for practical purposes. My object in formulating it is to show that the use of a diffusing screen in photometry is *necessarily* attended by an enormous reduction of light.

For our present purpose this loss of light is a serious matter. Weakened to $\frac{1}{25}$ by reflection from glass, the light of an ordinary candle or lamp is hardly sufficient to illuminate a diffusing screen properly, unless placed so close that measurement of the distances becomes uncertain. The difficulty might perhaps be got over by the use of incandescent electric lamps, but such were not at my command. When, as in Sir John Conroy's experiments,* the reflecting surfaces under test are metallic, or when (as above suggested) the observation relates to the transmission of light by an oblique plate of transparent material, the illumination given by a lamp may be adequate.

In my apparatus all the reflections are *regular*, and there is no further loss of light than the characters of the surfaces entail. An incidental advantage is that the accurate *flatness* of surfaces demanded by methods in which illumination is inferred from distance, is here unnecessary. The apparatus was first set up during the summer of 1885; but the glasses then at my disposal were not good enough, and when the parallel glass mirrors, &c., necessary for satisfactory working came into my possession, the season was so far advanced that I decided to postpone operations until the following summer.

* 'Roy. Soc. Proc.,' vol. 35, 1883, p. 26.

Description of Apparatus.

The light is admitted into the room through a pane of finely-ground glass fitted into the shutter. All other light is carefully excluded, and the walls and ceiling are blackened—an almost indispensable provision. The ground glass carefully cleaned* is illuminated not only by the direct light of the sky, but also by light from above reflected at a large mirror. The reception of light through a large angle not only favours the aggregate brightness and tends to moderate the changes due to passing clouds, but it makes the uniformity of the field more independent of the evenness with which the glass is ground. Under these circumstances, and when there is no sunshine, direct or reflected, falling upon the ground glass, the latter may be looked upon as a tolerably uniform source of diffused light. This uniformity, however, is not relied upon; but the arrangements are so made that the parts of the field compared are contiguous or identical, and are seen by rays which leave at the same angle. It will be convenient first to describe generally the course of the light, and afterwards the manner in which the adjustments were effected.

Proceeding from the ground glass (A), fig. 1, the light falls upon the transparent plates, B, B′, at which nearly equal parts are reflected. These plates are of worked glass about 6 inches by 4 inches, and are placed at the polarising angle. By this means are obtained two beams of polarised light of nearly equal, and of constant relative, brightness. The light transmitted by both plates is stopped at a screen, and takes no further part.

On the right hand side the light reflected at B is again reflected by a mirror of worked glass, silvered behind, at C, and assumes the direction CDF. D and F are alternative positions of the same mirror (also of worked glass, silvered behind). When the glass under test E is in use, the shifting mirror is in the position D, and the light follows the course CDEFH. In the contrary case, there is one reflection instead of two, and the ray takes the finally identical course CDFH. At H this central ray is reflected at the extreme edge of a speculum of silver-on-glass in the direction HI, to a small observing telescope, which is focussed upon this edge.

By adjustments that will presently be explained, it is secured that the reflections at D and F shall take place under the same angle, and therefore with the same (moderate) loss of light; but when E is in use the brightness is diminished some twenty times. To compensate this in the other position the disk G is introduced. It consists of a blackened disk of tin, from which (along a sufficient length measured radially) a sector is cut out, so that when the disk is

* Strong sulphuric acid is an excellent detergent for this purpose.

caused to revolve the view is cut off and blackness substituted for about nineteen-twentieths of the whole time. When the speed exceeds about twenty-five revolutions per second, there is no perceptible flickering, and the light is seen with a simple diminution of brightness. The idea of the method is so to adjust the angular opening that the effects of the glass under test and of the disk shall be equal.

The two brightnesses last considered can only be seen successively. They are separately tested with a comparison light, reflected at B′ from the same primary beam. After reflection at a silvered mirror, C′, and then at a blackened glass, D′, this light falls upon a silver-on-glass speculum at H′, and passes thence to the observing telescope at I. In setting up the apparatus a control over the brightness of the comparison light is obtained by varying the angle of incidence upon D′.

In order that the line of division between the two fields, as seen from I, may be quite sharp, it is necessary that the final reflector H be a speculum. To obtain a reflecting surface, perfect up to the very edge, a piece of silvered glass is carefully cut (on the glass side) with a diamond. If the operation is properly performed, the silver is left undisturbed, and when the plate is inclined, as at H, no part of the glass substratum is visible.

In adjusting the apparatus the object aimed at is to cause the central ray, ABB′, issuing from A on the ground glass, to assume ultimately the position HI, whether it proceed by the course on the left, or by either of the alternative courses on the right. As may be supposed, this is more easily said than done. All the reflectors require to be adjusted so as to be perpendicular to the plane in which the central ray is to travel. This plane is conveniently taken horizontal, so that every reflector has to be vertical.*

The central ray is defined by diaphragms at A, B, B′, points in the same horizontal straight line. At A the light is admitted through a small aperture only. At B, B′ the holes are cut in thin cardboard screens held in definite positions up to the glasses. It was found convenient to have them rather large—about half an inch in diameter. In setting up the apparatus the glasses B, B′, C are readily put into position, accuracy being required only in the levelling. The line CDF is now defined, and the next step is the more difficult one of fixing the two positions for the stand carrying the mirror D. This stand is (like all the others) provided with levelling screws, but these must not be used in passing from the one position to the other. A heavy

* The levelling of the reflectors was effected with the aid of the straight edge of a long board, adjusted until it coincided with the prolongation of its image. My assistant, Mr. Gordon, is expert at this adjustment of the edge to perpendicularity with the reflecting surface. The verticality of the latter is then tested by the application of a spirit-level to the edge of the board.

metal surface plate was laid down upon the table as the support for this stand, and carefully levelled. Under these circumstances the mirror if vertical in one position will remain vertical even though displaced; and this remains true, even though the feet of the stand do not rest immediately upon the plate, but upon small flat buttons of metal of uniform thickness, and perforated with equal holes, by which the feet of the stand are guided to definite positions. When the adjustments are complete, these buttons are fastened to the surface plate by dropping cement round their edges.

The position F may now be chosen at convenience, and without any particular care except in the levelling. The central ray, as fixed by the diaphragms, should fall near the middle of the surface. The other positions would also be somewhat arbitrary were it not for the necessity of securing the same angle of incidence and reflection in the two positions. To assist in this a small frame of brass wire is provided, carrying two pointers, and so arranged that it can always be placed in an absolutely definite position with respect to the mirror. By means of hooks it makes two contacts with the back, and two with the upper edge of the mirror. Of the other two contacts required to make up the necessary six, one is with the lower part of the face of the mirror, and the other with one of its vertical edges. Of the pointers, one (in the path of the incident ray) leads upwards, and the other (in the path of the reflected ray) leads downwards. By bending them suitably their extremities may be brought into the path of the central ray, so that when the eye is placed in such a position (H) as to see the central point A in the middle of the (apparently elliptical) aperture B,* this central point is just enclosed between the barely meeting pointers. By so choosing the second position, D, that this condition is again satisfied to an eye looking along ED, we secure not only the same angle of reflection but the use (for the central ray) of the same part of the glass. In making the adjustment we may first bring the pointer on which the incident ray strikes into the already determined line CD, and then rotating the apparatus about the vertical through this point, bring the second pointer to coincide with the reflected ray.

We have now to consider how to fix the position of E, the reflector under examination. Replacing the shifting mirror into its first position, F, we mark the line of the central reflected ray, FH, by needles, standing up from the table and as far apart as convenient. Transferring the shifting mirror to the position D, we have so to place E that the reflected ray shall coincide with the same line as before. For this purpose not only must the azimuth of E be correct, but its plane must be brought into the intersection of the already determined

* Auxiliary lighting, with a candle or otherwise, is sometimes necessary in order to see these apertures properly.

lines DE, FE. A levelled slab of glass is provided, on which to rest the feet of the stand carrying E. The mirror is now brought into a vertical plane, and may then be shifted on the slab without loss of this adjustment. The remaining double adjustment is best made systematically. By rotation about *any* vertical axis, the central ray may be caused to pass over one of the needles. If it fails to pass over the other, the axis of rotation must be shifted backwards and forwards until a suitable rotation allows satisfaction of both conditions. The ray now follows in both cases the course FGH, and the mirror H, with the sharp edge, may next be pushed in so as just to catch the ray in question and send it to the observing telescope (half of a small opera glass) at I.

The adjustments for the auxiliary light on the left hand side are a simpler matter. All the mirrors being levelled, the central ray is brought to the point H′, in the prolongation of IH. Nothing then remains but to turn the final (vertical) mirror round H′ until the reflected ray coincides with HI. When the eye looks in along this line, the bright spot should be seen in the same position from both mirrors.

To guard against accidental displacements, the movable pieces were usually secured with a little sealing wax. A diaphragm at K limits the field of view, and is so placed that the aperture is bisected by the division line H. It is not necessary to do more than allude to various screens employed to cut off stray light and render the room as dark as possible.

The principal trouble experienced, that of making and retaining the adjustments, is connected with the rather large scale of the apparatus, which made it difficult to use a single levelled bed for all the movable pieces. The question is thus suggested, what is it that fixes the absolute scale? And the rather unexpected answer must be—the diameter of the pupil of the eye, which is the only linear quantity concerned.*

In order to understand this it is necessary to bear in mind that although in describing the adjustments we speak of a single ray only, we are of necessity really dealing with a complete beam. The observation of a match requires that the two parts of the field of view have finite angular magnitudes, and from every point of the field there must proceed a pencil of rays limited by the pupil, or by the telescope. If all these rays are to be treated as sensibly parallel during their passage through the apparatus, certain limitations must be observed. For easy observation the field of view should subtend at the eye an angle of not less than a degree, so that if no telescope be employed the defect of parallelism must exceed this amount. The linear scale of the apparatus is not thus fixed, however, for we

* The wave-length of light may be regarded here as infinitely small.

might suppose the eye (armed when necessary with a focussing lens) to approach without limit the final mirrors. But if we do this we increase the defect of parallelism due to the aperture of the eye. It is true that we may elude the objection by contracting proportionally the effective aperture, but only at an expense of brightness, which cannot usually be afforded. In accordance with a universal rule, full brightness requires that the aperture of the eye be filled with light. In this way we see how it is that the aperture of the eye controls the size of the apparatus.

The employment of a telescope introduces a certain modification, which it may be worth while to state somewhat fully, as the principle is of general application. The extreme angle between the rays of the beam may be regarded as made up of two parts: (1) the angle subtended at the object-glass by the aperture in the diaphragm (K) near the final mirrors (upon which the telescope is focussed); (2) the angle subtended by the object-glass at the diaphragm. If—

a=diameter of pupil,
b=diameter of aperture in diaphragm,
r=distance between telescope and diaphragm,
m=magnifying power of telescope,
α=angular diameter of field of view presented to the eye,

then
$$\frac{mb}{r} = \alpha,$$

and the extreme angle between the rays of the beam—

$$= \frac{b}{r} + \frac{ma}{r} = \frac{\alpha}{m} + \frac{a}{b} \cdot \alpha.$$

We may here regard α and a as given beforehand; and we see that with a given b the first term may be reduced without limit by increasing m, and that then the defect of parallelism is proportional to a, the diameter of the pupil. If m and b can both be increased without limit, we may approach as nearly as we please to a state of things in which all the rays concerned are parallel. The preservation of full brightness throughout is already secured by the supposition that the effective aperture of the object-glass is ma.

The reasoning set forth above shows at any rate that the size of the apparatus cannot be reduced below a certain point, but I do not affirm that mine was not unnecessarily large. In addition to its other advantages, the use of a telescope gives facilities for obtaining a good focus upon the division line, an adjustment of great importance for the easy recognition of small differences of brightness.

The necessarily finite magnitude of the field of view involves a certain imperfection in this, and probably in other methods of photometry. We

can indeed secure that the lights seen in immediate juxtaposition come from the same part of the ground glass, but a corresponding perfection of adjustment does not apply to other parts of the field. If we suppose ourselves to be looking through the telescope at the ground glass, the part seen to the right of the division line really lies to the right on the ground glass. On the other side there is a distinction, according to the two positions of the shifting mirror. When the revolving disk is in use, the circumstances on the right hand side of the apparatus correspond to those on the left, and thus the part of the field seen to the left of the division line really comes from the left on the ground glass. The ground glass is thus seen much as if it were looked at directly, in spite of the separation of the light into two parts following distinct courses. On the other hand, when the additional reflector (under examination) is brought into play, there is another inversion, and the part of the ground glass seen to the left comes really from the right of the central line. In this case, therefore, it is the same part of the ground glass which is seen in both final mirrors. The distinction here pointed out would be of no consequence if the field were absolutely uniform, or if it were possible to compare the parts seen in immediate juxtaposition, without regard to the parts a little further removed. But if the original field vary slightly in brightness from right to left, it will be a question how far the eye would select for the match *continuity* of brightness across the division line, or how far it would demand equality in the *average* brightnesses of the two parts presented.

It now remains to describe certain accessories. During the observations it is necessary to have some means of varying the relative brightnesses of the two parts of the field without removing the mirrors or altering the width of the slit in the revolving disk. For this purpose a plate of glass (L), capable of rotation about a vertical axis, was introduced into the path of the light on the right hand side of the apparatus (between the second and third reflections). As the angle of incidence upon this plate increases, a greater proportion of the light is reflected and thrown away, and a less proportion is transmitted to the eye.

The observation consists in varying the azimuth of this plate until the match is satisfactory, after which the obliquity of the plate is measured. The transmission by the plate at the measured obliquity can then be found approximately from Fresnel's formula.* It may,

* A convenient table is given by Pickering ('Phil. Mag.,' vol. 47, 1874, p. 129). If A be the proportion of light reflected at a single surface, the transmission through a *transparent* plate is given by $(1-A)^2+(1-A)^2A^2+(1-A)^2A^4+\ldots\ldots=(1-A)/(1+A)$. The whole reflection is thus $2A/(1+A)$, from which Pickering's table is calculated. An erratum may be noted. For 65° the reflection should be 39·6, not 38·4, the value of A being supposed to be $\sin^2(\theta-\theta_1)/\sin^2(\theta+\theta_1)$, while $\sin\theta=1{\cdot}55\sin\theta_1$. There are some other minor inaccuracies.

perhaps, be objected that the use of this formula assumes the very thing that the experiments were principally intended to test; but the objection is evaded, almost if not altogether, when the aperture in the disk is so nearly adjusted to the ideal width that the oblique plate comes to take nearly the same azimuth for both sets of readings, *i.e.*, with and without the use of the mirror under examination. The use of the formula to allow for a small outstanding difference of obliquities can lead to no appreciable error. If on a first trial a large difference be found, a corrected aperture is calculated with the aid of Pickering's table, and the disk readjusted or replaced.

A fixed oblique plate has sometimes been used on one or other side of the apparatus in order to effect a rough adjustment of the brightness, and to bring the necessary obliquity of the rotating plate to a convenient amount (30°—60°). This was less trouble than a readjustment of the mirrors on the left, with an alteration in the angle of incidence upon the black glass D′.

In taking an observation the adjustment of the relative brightnesses was facilitated by a device which may now be described. If the attempt be made to secure an absolute match between the two parts of the field in view, a doubt is apt to arise as to whether the disappearance of the division line is due to the success of the adjustment or to fatigue of the eyes, leading, as in my case it very rapidly does, to imperfect focussing. This difficulty is less felt when the adjustment is under the immediate control of the observer, who can then satisfy himself of the sensitiveness of his eye by making the necessary displacement; but in the present experiments (on account of the distance of the telescope) it was convenient to employ an assistant. A glass plate, perpendicular to the path of the light, and attached to a sort of pendulum, was therefore provided on the left hand side, in such a manner that by pulling and letting go a string it could be introduced or withdrawn at pleasure. The effect of the plate would be to stop some 8 per cent. of the light, and the adjustment was so made that with glass *in* the (apparent) right of the field was as much too dark as it was too bright when the glass was *out*. The difference of brightness, amounting according to the above estimate to 4 per cent., was always fully apparent, and probably no setting more than 2 per cent. in error would be allowed to pass, giving, as such would do, a difference of 6 per cent. on the one side and of 2 per cent. upon the other.* Since the auxiliary light is eventually eliminated, it makes no difference, of course, whether we take for the comparison the full light, or the mean of the lights with and without the interposition of the plate.

A 2 per cent. error in single settings may lead to a 4 per cent.

* The accuracy of the settings falls much short of that attained by Professor Rood.

error in the comparison of the effects of the reflector and of the disk; and accordingly (since this may occur in either direction) an 8 per cent. discrepancy in the results is *possible*. This, however, would be very unlikely, and with a two- or three-fold repetition of the individual settings would be practically out of the question.

The revolving disks, used to diminish the light on the right hand side of the apparatus in about the same degree as by the mirror under test, were cut from tin plate, about 9 inches in diameter, and carefully centred. The angular apertures were finally calculated from measurements of the chord of the arc, and of the radius. It is important that the disks be thoroughly blackened, in view of the assumption that no light reaches the eye except during the passage of the aperture. Here is one reason why it is desirable to keep the room as dark as possible. The disk should also be properly balanced. On one occasion a curious and at first puzzling effect was observed. The division line, which should present no visible width, sensibly widened, appearing sometimes darker than the nearly balanced adjoining fields, and sometimes, though more rarely, appearing relatively bright. The explanation is to be found in a vibration of the mirror, whose edge forms the division line, in a horizontal direction perpendicular to the line of sight, the vibration being communicated from the revolving wheel through the floor to the table upon which the mirrors stood. It is evident that if the two lights under comparison were equal, not merely on the average, but at every moment of time, such a movement of the mirror would have no disturbing influence, and could not make the division line visible. But it is otherwise when one of the lights is intermittent, and the vibrations of the mirror are executed (as here they must be) in the same period. For suppose that at the moment when the division line is advanced, so as to invade still further the field from the back mirror, the light is reaching the eye through the aperture in the disk. In this case the parts near the edge of the vibrating mirror will be sending to the eye the full light due to this part of the field. During the remainder of the vibration, no light should reach the eye, but if this mirror retreats, the back mirror sends its continuous light from the same apparent place, so that when the angular opening in the disk is small, it is possible for the part of the field over which the division line vibrates to present an almost doubled brightness, combining in fact the illumination of the two parts of the field. A different phase relation may evidently lead to an abnormal diminution of brightness in the same region. These effects disappeared when the disk was better balanced.

Prism of Crown Glass (I).

In ordering a glass for the purpose of determining the reflecting power of a surface, a prism was preferred to a plate, both on account of the easier separation of the reflections from the front and back surface, and also because the refractive index could be determined more readily. During the observations the hind surface was coated with black varnish, the effect of which, however, in annulling the second reflection, was far from complete.

With this glass, carefully cleaned (but not repolished), six sets of observations were made, four by myself and two by Mrs. Sidgwick. Each set consisted of three or four settings with the glass in operation, and about the same number with substitution of the revolving disk. The following is a set of readings by myself on August 7th, 1886:—

Face of Prism (I).

Reflection.		Revolving disk.
39°·0	.....	40°·3
38·2	.....	42·7
37·8	.....	43·0
39·1	.....	42·0
—	.....	43·5
Mean.. 38·5	.....	42·3

The angles here given are the obliquities of the adjustable glass plate used to graduate the intensity. According to Pickering's table, calculated from Fresnel's formula ($\mu = 1{\cdot}55$), the effect of this plate at 38·5° would be to reflect 15·7 per cent. of the light incident upon it. The light transmitted is therefore 84·3 per cent. In like manner the light transmitted by the plate at an obliquity of 42·3° is 82·5 per cent.

In order to complete the calculation of the reflecting power of the glass surface, we must know the proportion of light transmitted by the revolving disk. Measurements gave for the chord of aperture of this disk in fiftieths of an inch 45·0, corresponding to a radius of 174·25. The angle of aperture is thus 14° 50′ = 14·83°. Accordingly the factor expressing the reduction of light by use of the disk is—

$$14{\cdot}83/360 = 0{\cdot}04119.$$

The reflection from the face of the glass prism is thus—

$$\frac{82{\cdot}5}{84{\cdot}3} \times 0{\cdot}04119 = 0{\cdot}0403.$$

The following is a summary of the results obtained at this time:—

Face of Prism (I).

Lord Rayleigh.			Mrs. Sidgwick.	
Aug. 4....	0·0411	.....	Aug. 4....	0·0405
„ 5....	0·0413	.....	„ 5....	0·0413
„ 7....	0·0403	.....		—
„ 9....	0·0413	.....		—
Mean..	0·0410	.....	Mean..	0·0409

Final mean..... 0·04095.

Since this number is very nearly the same as that (0·04119) due to the disk alone, we see that the result is scarcely at all dependent upon the correctness of the assumed effect of the oblique plate.

It now remains to make comparison with the reflection as given by Fresnel's formula, viz.:—

$$\frac{\sin^2(\theta-\theta')}{\sin^2(\theta+\theta')}, \quad \ldots\ldots\ldots \quad (A)$$

where $\sin\theta/\sin\theta'=\mu$, and μ is the refractive index.

The index was determined in the usual way from the angle of the prism (i) and from the minimum deviation (D). The value of i was found to be 9° 50′. The minimum deviation of soda light on one side was 5° 5′, and on the other 5° 4½′. Thus D = 5° 4¾′, and

$$\mu = \frac{\sin\frac{1}{2}(D+i)}{\sin\frac{1}{2}i} = 1{\cdot}5141.$$

The angle of incidence (θ), which was the same in all the observations with the various reflectors, was measured by determining the angle (2θ) between the incident and reflected ray. This measurement does not require great precision, for a change of a whole degree in the value of θ would alter the reflection about 1 per cent. only. I found—

$$\theta = 13°\ 52'.$$

With these values of θ and μ, we find—

$$\frac{\sin^2(\theta-\theta')}{\sin^2(\theta+\theta')} = 0{\cdot}04514,$$

about 10 per cent. in excess of the reflection actually observed.

In order to satisfy myself that the deficiency of reflection was real and permanent, this prism was remounted after a thorough cleaning, and further observations were taken, as summarised in the following table. The revolving disk was the same as before:—

Prism of Crown Glass (I), Remounted.

	Lord Rayleigh.		Mr. Gordon.		Mean.
Aug. 24	0·04085	....	0·04100	....	0·0409
„ 25, morn...	0·04183	....	0·04199	....	0·0419
„ 25, even.*.	0·03950	....	0·04030	....	0·0399
„ 26	0·04190	....	0·04170	....	0·0418
Mean....	0·04102	....	0·04125	....	0·04113

The difference between 0·04113 and the mean previously found, viz., 0·04095, has no significance.

In consequence of the detection of a greatly augmented reflection from another glass surface (II, below), as the result of a repolish with putty powder, this surface also was submitted to similar treatment. Immediately afterwards, on August 30th, a much increased reflection was observed, the numbers by two observers being—

0·0481, 0·0472; mean, 0·0476.

The disk gave, as before, a transmission 0·04119; so that the numbers for the repolished face depend too much upon the assumed effect of various obliquities of the inclined plate to be fully trustworthy, even were they sufficiently numerous to guard against accidental errors. But they proved, unequivocally, a considerable increase in reflecting power as the result of the repolish.

In view of these results, a new disk was prepared of angular aperture about $17\frac{1}{4}°$, and, consequently, with a transmission equal to 0·04763. The numbers obtained with this are shown in the following table:—

Prisms of Ground Glass (I) Repolished.

	Lord Rayleigh.		Mr. Gordon.
Aug. 30, aft.'.....	0·0461	....	0·0451
„ 31, morn.....	0·0451	....	0·0454
„ 31, aft.....	0·0448	....	0·0447
Mean	0·0453	....	0·0451

Final mean = 0·0452.

The observed result now agrees remarkably well with that calculated from Fresnel's formula; but unfortunately it depends more (for about 5 per cent. of its value) than could be wished upon the use of the oblique plate.

* The considerable discrepancy shown in this set of readings was probably caused by insufficiency of light.

Prism of Crown Glass (II).

So soon as it appeared that the reflection from the face of prism (I) fell so much short of what was to be expected in accordance with Fresnel's theory, I tried another prism whose surface was still older than that of (I). The event proved a still more marked deficiency. With the aid of the disk giving transmission 0·04119, the following numbers were obtained:—

Prism (II), before Repolishing.

	Lord Rayleigh.		Mrs. Sidgwick.		Mr. Gordon.
Aug. 26..	0·0349	....	—	....	0·0344
„ 27..	0·0342	....	0·0350	....	—

Mean.. 0·0346.

Although somewhat dependent upon the assumed effect of the oblique plate, this number is far too low to be consistent with anything obtainable from Fresnel's formula with an admissible index.* This circumstance suggested a repolishing of the surface, which, however, was superior to that of (I), so far as could be judged from close inspection in a favourable light. The repolishing was executed by Mr. Gordon by means of a disk of wood charged with putty powder and mounted in the lathe. Observation now demonstrated a remarkable improvement in the reflecting power, as the following numbers will show:—

Prism (II), after Repolishing.

	Mrs. Sidgwick.		Lord Rayleigh.		Mr. Gordon.
Aug. 28, morn...	0·0491	....	0·0488	....	—
„ 28, aft.....	—	....	0·0479	....	0·0473
„ 30, morn...	—	....	0·0484	....	0·0481

Mean.. 0·0483.

Here again too much depends upon the oblique plate, the transmission of the disk being only 0·04119; but there can be no doubt of an increase in the reflection of something like 30 per cent. If we may argue from the number obtained from prism (I) after repolishing,

* If e^2 be the proportions of light reflected at incidence θ, then Fresnel's formula is equivalent to—

$$\tan\theta_1 = \frac{1-e}{1+e}\tan\theta,$$

by which θ_1 is found. The index is then given by $\sin\theta/\sin\theta_1$. In the present case

$$e^2 = 0{\cdot}0346, \quad e = 0{\cdot}1860, \quad (1-e)/(1+e) = 0{\cdot}08140/1{\cdot}1860,$$

so that, since $\theta = 13^\circ\ 52'$, $\theta_1 = 9^\circ\ 37'$, $\mu = 1{\cdot}434$.

under nearly similar circumstances, viz., 0·0476, we may conclude that the true reflecting power of this prism is about 0·0460.

Altogether the evidence favours the conclusion that recently polished glass surfaces have a reflecting power differing not more than 1 or 2 per cent. from that given by Fresnel's formula; but that after some months or years the reflection may fall off from 10 to 30 per cent., and that without any apparent tarnish.

The question as to the cause of the falling off, I am not in a position to answer satisfactorily. Anything like a disintegration of the surface might be expected to reveal itself on close inspection, but nothing of this kind could be detected. A superficial layer of lower index, formed under atmospheric influence, even though no thicker than $\frac{1}{100000}$ inch, would explain a diminished reflection. Possibly a combined examination of the lights reflected and transmitted by glass surfaces in various conditions would lead to a better understanding of the matter. If the superficial film act by diffusion or absorption, the transmitted light might be expected to fall off. On the other hand, the mere interposition of a transparent layer of intermediate index would entail as great an *increase* in the transmitted as falling off in the reflected light. There is evidently room here for much further investigation, but I must content myself with making these suggestions.

Plate Glass Silvered Behind.

This glass was silvered chemically by the milk-sugar process, and by transmitted light showed the sky of a normal deep blue colour. The film was not polished. In determining the efficiency of this and other good reflectors, the black glass mirror D′ was replaced by one silvered behind. The first trial without a revolving disk gave for the reflecting power 0·82. This result, of course, depended entirely upon the assumed influence of various obliquities of the adjusting plate. A disk was therefore prepared with two opposite projecting teeth, in which the ratio of aperture to circumference turned out on careful measurement to be 0·8230. This number, therefore, represents the transmission of light by the disk. Using this disk I found the following values for the reflecting power of the mirror for light incident upon it at an angle of 13° 52′:—

Aug. 11.....	0·823
„ 12.....	0·833
Mean.....	0·828

This result relates, like all the others, to light polarised in the plane of incidence. Mirrors of this kind are durable, and not being exposed to tarnish are more convenient than specula, whenever the double reflection is not objectionable. The high reflecting power is a satisfactory feature.

Silver-on-Glass Speculum.

This was the silver side of the same glass as the last, polished with wash leather and a little rouge. The milky film was not perfectly removed. Four observations, not over concordant, probably in consequence of variation of reflecting power at different parts of the surface, gave—

Lord Rayleigh.		Mrs. Sidgwick.		Mr. Gordon.
Aug. 14.. 0·902	....	Aug. 16.. 0·933	....	Aug. 14.. 0·920
„ 16.. 0·895				

Mean.. 0·912.

The surface was then repolished and remounted with the following results:—

Lord Rayleigh.		Mr. Gordon.
Aug. 18.... 0·950	.....	Aug. 18.... 0·952
„ 19.... 0·938	.....	„ 19.... 0·911
—	.....	„ 21.... 0·938
Mean.... 0·944	.....	Mean.... 0·934

Mean.. 0·939.

The increase in efficiency may have been due to a more careful selection of the best polished central part as much as to actual improvement in the polish of the speculum as a whole. The transmission of the disk used with this surface is 0·9105.

Sir John Conroy* found an even higher number (0·975) as the reflecting power of silver films for light polarised in the plane of reflection, and incident at 30°.

Mirror of Black Glass.

A plate of opaque glass has the advantage that the influence of the hinder surface is eliminated without more ado; but, on the other hand, it lends itself less readily to determinations of index. The following results were obtained with such a plate:—

	Mrs. Sidgwick.		Lord Rayleigh.
July 29....	0·0580	.....	—
„ 30....	0·0581	.....	0·0570
„ 31....	0·0583	.....	0·0572
Aug. 2.....	0·0574	.....	0·0578
„ 3.....	0·0581	.....	0·0577
Mean ...	0·0580	.....	0·0574

Final mean.. 0·0577.

* 'Roy. Soc. Proc.,' vol. 37, 1884, p. 38.

During these observations a disk was employed giving transmission 0·0577, so that in this case the final result is absolutely independent of the effect of the adjustable oblique plate. It will be observed that the separate results obtained by Mrs. Sidgwick and by myself differ, even in the means, by 1 per cent. This is not the only instance in which the errors have presented a suspiciously systematic appearance; but the differences being always small could not be submitted to any satisfactory examination. It rarely happened, for instance, that Mrs. Sidgwick and I could find definite fault with each other's settings.

When these results were first obtained, I thought that they would turn out to be too high for agreement with Fresnel's formula, supposing that the index of the glass was low. A subsequent measurement of the specific gravity, however, gave reason for suspecting that the glass might be flint, a conclusion confirmed by determinations of the refractive index.

These were made by two methods: (1) by observation of the polarising angle in air, (2) by observation of the angle at which total reflection sets in when the mirror is immersed in bisulphide of carbon. The first is, perhaps, the simpler in respect of experimental arrangements, but it is open to the objection that the inference of the refractive index from the polarising angle is somewhat theoretical.

The black glass was mounted upon the turntable of an ordinary goniometer. In the focus of the collimator was placed a wire, seen dark in a bright field of view. Various positions of the turntable were then tried, such that on rotating a Nicol held at the eye the dark patch appeared to pass somewhat to the right or to the left of the collimator wire. After each observation the web of the telescope was set to coincidence with the collimator wire, and a reading taken. Success depends in some degree upon the use of a suitable light. Sunshine diffused through ground glass answered the purpose very well.

Right.		Left.		Central.
64° 25′	....	63° 56′	....	64° 7′
16	....	40	....	64 0
17	....	55	....	—
14	....	55	....	—

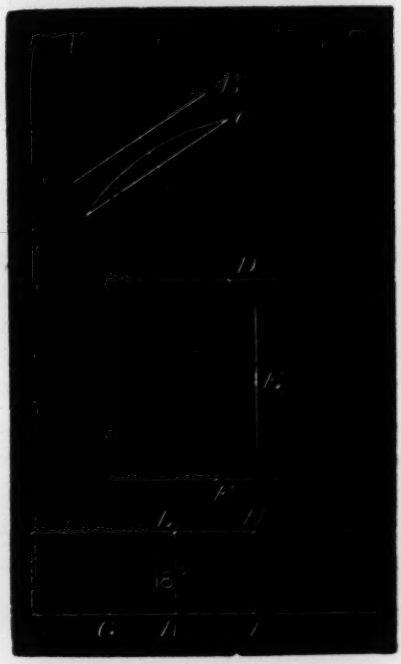
The table gives a set of circle readings. In the first column the patch was to the right of the collimator wire, in the second to the left, and in the third there was no appreciable deviation. We may, therefore, take as the reading for the polarising angle 64° 5′, with a probable error not exceeding 3′ or 4′. The reading for a direct setting of the telescope upon the collimator wire was −5′, so that the polarising angle is $\frac{1}{2}(180-64° 10′)=57° 55′$. Whence according to Brewster's law—

$$\mu = \tan 57^\circ\ 55' = 1{\cdot}5952.$$

This relates to white light.

To find the index of refraction by the method of total reflection, the mirror was mounted vertically in a small tank of plate glass, cemented with glue and treacle, and containing bisulphide of carbon. The mirror E, as shown in fig. 2, was parallel to one of the sides of

Fig. 2.



the tank, and a cover was provided to check evaporation. A uniform field of homogeneous light could be obtained from a salted spirit lamp, A, with the aid of a plate of ground glass, B, and a collimating lens, C. The eye looking in along such directions as GF, is able to mark with considerable accuracy the direction in which total reflection begins. By the aid of plumb-lines, &c., this direction and that of the face of the mirror (seen from above) were marked upon a board, and it appeared that the angle GLK, between the face of the mirror and the direction GLF of the first totally reflected emergent light was 18°.

A beautiful variation in the experiment may be made by replacing the spirit lamp with a candle, and subsequently analysing the reflected light by a direct vision prism. For this purpose a screen carrying a slit should be interposed as near F as conveniently may be. As the incidence of the light upon the black glass becomes more grazing total reflection sets in, but first at the violet end of the spectrum.

When the eye is looking nearly in the right direction, the spectrum appears to be covered by a veil proceeding from the red end up to a point dependent upon the precise direction of the light. By slightly shifting the eye, the veil may be made to reach any desired part of the spectrum, and then we know for what ray total reflection is just commencing. By bringing the veil to touch the soda line (rendered visible with the aid of the spirit lamp), precisely the same direction was found as had previously been marked out with use of homogeneous light. It would be possible in this way to determine with considerable accuracy the dispersive powers of opaque bodies.

The angle of 18°, being measured in air, is not the complement of the true angle of reflection. If we take 1·630 as the index of CS_2 for soda light, we find for this angle

$$\sin^{-1}\left(\frac{\sin 18^\circ}{1\cdot630}\right)=10^\circ\ 56';$$

whence for the index of the glass relative to soda light,

$$\mu=1\cdot630\cos 10^\circ\ 56'=1\cdot600.$$

The amount of reflection according to Fresnel's formula, with an incidence of 13° 52′ and an index 1·600, is 0·05726, a little *less* than that actually observed. The agreement is as good as could be expected, but it should be noticed that this mirror was merely cleaned and not repolished with putty powder. If repolishing were to produce as much effect in this case as upon the acute-angled prism (I), Fresnel's formula would be left considerably in arrear.*

P.S. Nov. 9, 1886.—I am indebted to Mr. Glazebrook for a determination of the refractive index of the prism of crown glass II. He finds $\mu = 1\cdot5328$. The introduction of this into Fresnel's formula ($\theta = 13^\circ\ 52'$) gives for the reflecting power 0·0477.

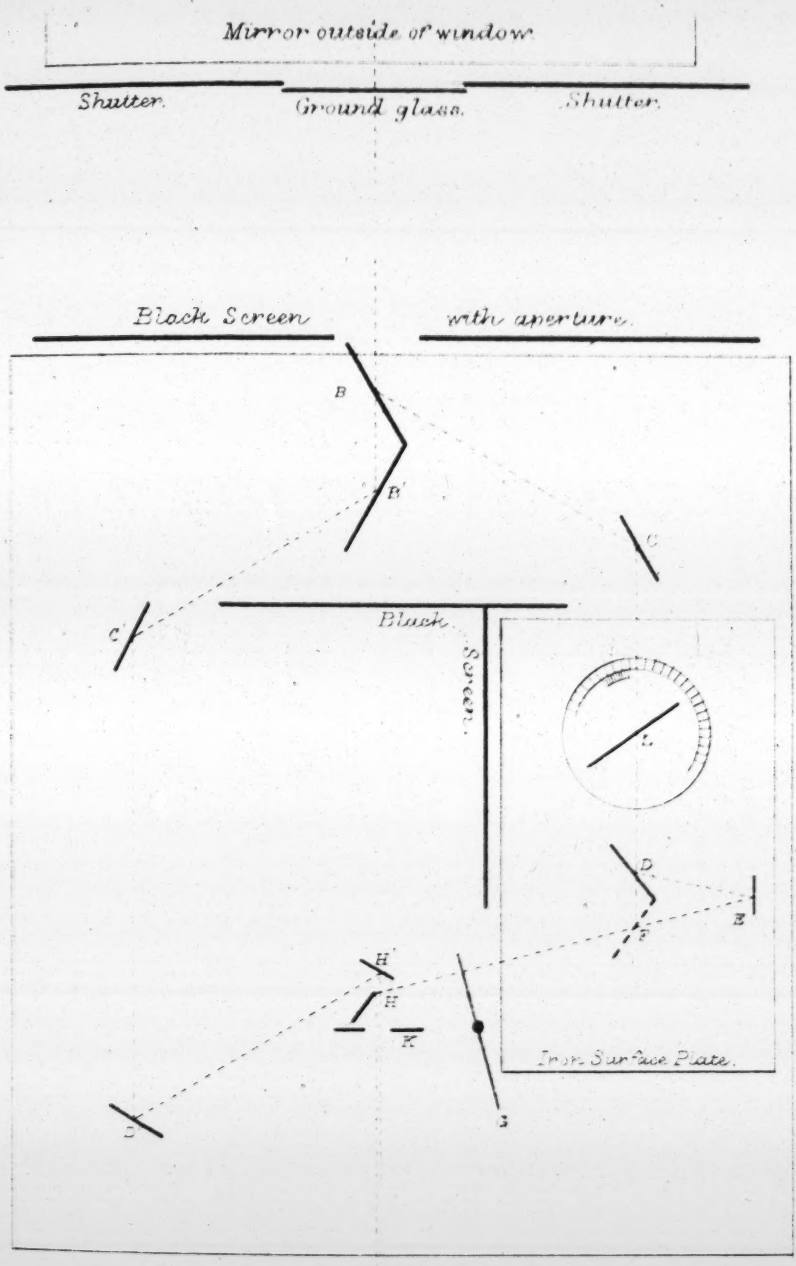
V. "A Theory of Voltaic Action." By J. Brown. Communicated by Lord Rayleigh, Sec. R.S. Received October 4, 1886.

[Plates 4 and 5.]

1. From a series of experiments made more or less continuously during the last five years the following conclusions have been drawn:—

That the difference of potential near two metals in contact as observed either by the bi-metallic condenser (Volta's) method,

* Some of the results here given were communicated to the British Association at Birmingham, where also was read a paper by Sir John Conroy on the same subject.



Scale = $\frac{1}{12}$

West, Newman & Co. sc.

or by the bi-metallic ring or quadrants method of Sir William Thomson, is due to the chemical action of a film of condensed vapour or gas on the surfaces of the metals.*

That the two metals with their liquid or quasi-liquid films are quite similar to a galvanic cell composed of the same metals as elements, and a liquid similar to that of the films as electrolyte; the said electrolyte being (in the ordinary static "contact" experiment) divided by the intervening insulating diaphragm of air or other gas.

2. Therefore in these experiments it is the difference of potential at the outer surfaces of the two films that is measured.

In the case of a single metal, *e.g.*, zinc covered by its chemically active electrolytic film, there is, according to the view here advocated, at the surface of contact between the film and the metal, an electromotive force due to the chemical action between them, charging the zinc negatively and the film positively. The two charges being bound have no outer manifestation of any kind.

Copper again, for example, is affected similarly, but less strongly, than zinc. If it be connected metallically to the zinc, the potentials of the metals will be equalised, a portion of the negative charge going through the connexion from the zinc to the copper and thence to the film on the copper.

3. This theory as applied to the Volta condenser experiment is referred to somewhat in detail by Wiedemann ('Die Lehre von der Elektricität,' vol. 2, pp. 986—987); and the view held by De la Rive ('Traité de l'Électricité,' vol. 2, 1856, p. 776) was very similar, differing mainly in the supposition that the positive charge separated off in the film was maintained there by the insulating nature of the film, whereas it is evident that an electromotive force at the surface of contact of the film with the metal sufficient to cause the separation of the two charges will also be sufficient to keep them separate. We are, therefore, in so far not debarred from considering the film an electrolytic conductor. De la Rive's view seems, however, to have received little general attention, perhaps less than it deserved.

4. It seems advisable to discuss these views experimentally while trenching as little as possible on the uncertain domain of speculative molecular physics, and in describing the experiments I shall refer in detail to those only which gave reliable results. Many preliminary trials were made before disturbing causes were eliminated, and many negative results were obtained. It has, however, been gratifying to find that no reason has appeared requiring the alteration of anything in the two short papers previously published on this subject ('Phil. Mag.,' Aug., 1878, Feb., 1879).

* [No account is here taken of any infinitesimal effects that may be due to thermo-electric action, &c.—December 3, 1886.]

5. The principal piece of apparatus used was a bi-metallic quadrant electrometer represented at fig. 1, about one-fourth actual size. Its design was an elaboration of the temporary apparatus described in a previous paper ('Phil. Mag.,' vol. 2, 1878, p. 142). A is a metal case supported on levelling screws; B a thick vulcanite tube provided at its upper end with a brass cap containing a collar of leather in which slides the brass rod C. D is a brass ball with clamping screw to retain the rod at any desired height. E is a platinum torsion wire, 0·001 inch diameter, carrying the stouter wire F, on which are fixed a light concave mirror G, the "needle" H, and a glass weight J dipping in water or other liquid contained in a glass vessel, resting on a vulcanite plate on the bottom of the instrument. R and L are the quadrants of the metals under examination screwed to a vulcanite piece M which is adjusted by hole, slot, and plane on the points of three long fine-threaded levelling screws, O, P, Q, passing up through stuffing-boxes in the bottom of the instrument. The quadrant L is connected to an insulated wire N passing out air-tight through the back of the case. R is connected to the body of the instrument and to "earth" by a slip of foil under the vulcanite support touching the screw O. A stopcock T in the top of the vulcanite tube and another in the bottom of the instrument of which the opening is shown at S, provided means for varying the nature of the gas surrounding the metals. The opening in front of the instrument was provided with a flange, faced up to a plane, on which a pane of plate glass was jointed by grease or other suitable means. The needle was electrified either positively or negatively by connecting its suspending wire through a Pohl's reversing commutator to either pole of a battery of 100 small Daniell cells: the opposite pole being at the same time connected to the body of the electrometer and to "earth."

6. To make an experiment with this apparatus the quadrants were first carefully adjusted with their upper surfaces in the same plane, then put in position, roughly levelled, the connexion to wire N made, and the pair dusted with a camel-hair brush. The needle which had been raised by the sliding-rod C was now lowered to within about 0·05 inch of the surface of the quadrants, and adjusted with its centre line over the slit between them and its suspension wire in the centre of the system.

If now on connecting the quadrants metallically, and electrifying the needle positively and then negatively, it was found that the deflection (as observed by lamp and scale) was greater on one side of zero than on the other, due to one quadrant being higher than the other, the necessary adjustment was made by means of the screws O and P. A second concave mirror fixed on the ball D was now clamped, so that its reflected beam also fell on the zero of the scale,

so that after raising the rod C it could be readily brought back to its original position. The quadrants were now levelled by a spirit-level from front to back, after which the difference of potential might be at once taken, or they might be removed again to be cleaned, rapidly replaced, and a reading taken as quickly as possible afterwards without the loss of time in adjusting them. A "reading" was taken by causing the needle (by reversing its potential twice) to deflect first on one side (say right) of zero, then to the left, and again to the right. The mean of the two right-hand deflections was added to the left-hand one in order to eliminate the effect of a slow shifting of the zero which sometimes occurred, caused apparently by a slow untwisting of the suspension wire, or a slight warping of the vulcanite piece M.

7. The readings were either qualitative when the two quadrants were put in metallic connexion, or quantitative when the difference of potential of their films was compared with a standard cell.

In order to make the connexions conveniently for both kinds of readings a modified Pohl's commutator was used, by means of which the quadrants could either be directly connected with each other, or else one to each pole of the standard cell W in either sense.

8. At first two standard cells were used, a Clark's cell purchased from Elliot Brothers, and a Daniell constructed as at W. X, Y, Z are three test-tubes fixed in a block of paraffin. X contains a half-saturated solution of zinc sulphate, with a rod of redistilled zinc not amalgamated. In Z is a gutta-percha covered copper wire, having its lower end cleaned, wound into a spiral, and covered with crystals of copper sulphate and water. Y contains distilled water with a strip of zinc to reduce any copper salt that might diffuse over from Z.

When the Clark's cell was used, the results were reduced to the Daniell as unit value. Comparisons with both cells were at first frequently made.

9. The measurement of the difference of potential near any pair of quadrants was made by a well-known method as follows. The cell was first connected, so that its electromotive force should increase the deflection due to the difference of potential near the quadrants, and a reading a taken. The connexion was then reversed, and a reading b taken. If d is the deflection due to the standard cell, and p that due to the difference of potential near the quadrants—

$$d+p=a$$
$$d-p=b$$

$$\text{therefore} \quad \frac{p}{d}=\frac{a-b}{a+b}.$$

For example, in comparing the difference of potential of the films on slightly tarnished copper and zinc in air the following deflections were found—

Copper of cell to zinc quadrant—
Right, 76; left, 75; right, 74·5. a=150·25.
Copper of cell to copper quadrant—
Right 19·5; left, 11·5; right, 20·5. b= 31·5.
Whence p=0·65 Daniell.

Owing to the nature of the apparatus, extreme accuracy in the results as absolute quantitative measurements is not to be expected. They are meant in most cases more for comparisons among themselves.

10. A number of experiments were made to ascertain the rate of decrease of difference of potential of the films on copper and zinc in air due to gradual tarnishing of the metals. The curves corresponding to two of these with zinc of different degrees of purity are given in fig. 2, where ordinates are in decimals of the electromotive force of one Daniell, and abscissæ represent the time elapsed after cleaning the metals with glass paper. Ten more observations were made on the pair of copper and redistilled zinc giving curve A. The remainder of the curve has the same character, falling in 61½ hours to 0·64 Daniell.

The object was to estimate the difference of potential due to the clean metals by carrying the curve back to the time at which they were cleaned, and compare this with the difference of their heats of combustion in oxygen as compared with the heat equivalent of the standard cell. The result agrees fairly well with the thermochemical data of J. Thomsen ('Wiedemann, Beiblätter,' 1880, Nos. 7 and 8).

We are met, however, with the fact that with many other pairs of metals in air we do not find the same agreement, although in most cases there is a more or less near approximation. This I believe is due to two causes:—

(1.) We cannot tell the exact nature of the chemical process going on at the surface of the metals, whether it be simply an oxidation; which particular oxide of the metal is formed, or if the other gases, ammonia, carbonic acid, &c., take any part in the process, nor do we know exactly the physical state of the film.

(2.) We do not know how much protective effect the already formed coating of oxide, &c., exercises on the metals.

11. The comparatively small and often irregular difference of potential observed when experimenting with copper and iron may be due to the formation on the latter, immediately after cleaning, of a highly protective oxide coating, producing a more or less passive state analogous to that which this metal assumes under other well-known conditions. The rapid tarnishing of lead may also produce a similar effect supposing the oxide formed be of a very coherent and protective nature.

The consideration as to which oxide is formed leads to the question whether any difference of potential would be found near the contact of two different oxides of the same metal—I experimented on litharge and red lead, and on stannous and stannic oxides, but did not get results sufficiently definite to be reliable—but I think the experiments worth repeating with freshly-formed oxides, which mine were not.

12. It has been suggested in criticising my former experiments on the effect of various gases on the difference of potential near certain metals in contact, that it would have been better to have had quantitative results. Although for the reasons given above these are perhaps not of very much value, I have, however, made measurements in several cases. The following are the results with a copper-iron pair in hydrogen sulphide gas.

Quadrants of these metals after being freshly cleaned with glass-paper were put in position, and the difference of potential in air measured as quickly as possible was found to be 0·059D, 0·062D, mean 0·06D, iron side positive.

Hydrogen sulphide was now sent into the electrometer, reversing immediately the relative potentials near the metals. The stopcocks were then closed and the following measurements taken:—t representing the time elapsed after admission of the gas, and P/D the ratio of the difference of potential near the metals to that of the terminals of a Daniell cell.

t.	8 m.	15 m.	25 m.	1 h. 18 m.	2 h. 10 m.	2 h. 35 m.	12 h. 10 m.
P/D.	0·4	0·48	0·53	0·55	0·47	0·43	0·3

Some fresh hydrogen sulphide now sent in caused a slight rise, P/D=0·32, after which it fell slowly, and in about 11 hours more (t=23 h. 25 m.) it was 0·26. The instrument was now cleared of gas and left open for about 1½ hours, when P/D fell to 0·1. Re-admission of gas raised it to 0·32. This was repeated with the same result, after which the instrument was left open for 24 hours, when the iron side was found positive, P/D=0·32. This somewhat high value may be due to oxidation of the iron sulphide, but more probably perhaps to oxidation of the iron itself, coupled with the protective action of the very thick coating of sulphide on the copper. After again admitting the gas the copper side became again positive, P/D=0·26D. Several more changes of the atmosphere were made, the corresponding change in difference of potential taking place each time. The electrometer was then charged with the gas and closed up, the water vessel having been removed (so that its vapour might not interfere with insulation inside the instrument), and its place supplied by paper vanes cemented to the weight. The difference of potential then fell gradually till at the end of eight days after the gas had first acted on the metals it was sensibly zero. On opening the electrometer plenty

of the hydrogen sulphide was found still present in it, and the metals were much corroded; the copper brown with lighter patches; the iron greyish with patches also, but having a thinner coating of sulphide than the copper. On allowing the metals to remain exposed to the air, the iron side became again gradually more positive; the difference of potential rising during 26 hours to 0·26D, after which it fell in about a week to 0·22D.

13. As the electrochemical relation of silver and iron in water is also reversed when solution of potassium sulphide is added, I had little doubt that an addition of hydrogen sulphide to the air about these metals when in contact would reverse the potentials near them.

To test this the upper surface of the copper quadrant used in the last experiment was covered with a piece of thin silver sheet cemented on, thus forming a silver-iron pair with the iron quadrant. The difference of potential just after cleaning the metals with emery- and glass-paper was 0·23D, iron side positive. In three minutes after admission of the gas the next reading showed the silver side positive; difference about 0·4D. The following readings were then taken, t as before the time after admission of the gas.

t.	12 m.	16 m.	1 h. 0 m.	23 h. 40 m.	33 h. 30 m.	58 h. 40 m.
P/D.	0·42	0·44	0·43	0·38	0·35	0·35

84 h. 20 m.	94 h.
0·365	0·38

The sulphide formed on the inside of the metal case of the instrument by the action of the gas, now began to peel off in scales and jammed the vanes of the weight. After cleaning these out fresh gas was sent in, and P/D became 0·36D; silver side still positive. Measurements were then made at intervals, the difference of potential falling gradually to 0·23D, on the fourteenth day after admission of the gas at the outset.

By that time, however, the gas was found to have been all or nearly all absorbed by combination with the metals and the sides of the instrument. The conclusion of the experiment is therefore incomplete, but it indicates that the difference of potential decreases with these metals also, although more slowly than with copper and iron.

14. It was suggested by Mr. Cross, of Barrow-in-Furness, that the difference of potential near a copper-iron pair in air and in ammonia gas should be compared. The electromotive force of a cell formed by these metals immersed in distilled water is about 0·35D, copper the positive pole, but if ammonia solution be added, this electromotive force reverses and becomes 0·27D.

Three experiments were made with copper-iron quadrants in this gas, which was sent into the instrument by boiling strong liquor

ammoniæ in a flask connected by glass tubing to the stopcock T. In each case the potential was reversed, and the difference of potential appeared rather less in the ammonia atmosphere than in air, but owing to the action of the gas on the vulcanite piece M, causing it to twist and so alter the position of the quadrants, I was unable to get reliable quantitative comparisons with the standard cell.

15. In a cell composed of copper and nickel in distilled water, nickel is the positive metal, and the electromotive force is about 0·24D. On adding a few drops of a solution of ammonia a reversal takes place and copper becomes the positive metal. The electromotive force appears at first rather greater than that of the water cell but falls off quickly.

The analogous experiment with gaseous ammonia was made in a qualitative way only. The quadrants of the electrometer were replaced by a flat ring half copper and half nickel, the two metals soldered together. (This had been used in a former experiment with hydrochloric acid gas.)

The metals of the ring having been rubbed bright with emery-paper, the potential in air only near the nickel was positive to that near the copper, the index passing over about $4\frac{1}{2}$ divisions, when the electrification of the needle was reversed.

After admitting ammonia gas in the same way as before described the copper side became positive, at first slightly, but after two hours the index moved over six divisions for a reversal of the needle. The two kinds of experiments therefore agree here also fairly well.

16. I now sum up the results (regarded qualitatively only) obtained in this kind of experiment, where a change in the constituents of the atmosphere surrounding a pair of metals in contact reverses the difference of potential near them in correspondence with the reversal of electromotive force, which takes place after a similar change in the corresponding liquid electrolyte used with the same metals as a voltaic cell.

Pairs of metals.	Compound whose addition in solution causes a reversal of electromotive force in a hydro-element or cell.	Compound whose addition as gas to the atmosphere surrounding the metals in contact causes a reversal of the difference of potential observed near them.
Copper-Iron	Potassium sulphide	Hydrogen sulphide.
Copper-Iron.....	Ammonia.....	Ammonia.
Silver-Iron	Potassium sulphide.....	Hydrogen sulphide.
Copper-Nickel ...	Ammonia.....	Ammonia.
Copper-Nickel ...	Hydrochloric acid.....	Hydrochloric acid.

These are the only cases tried; no exception has, therefore, been found by me, nor, so far as I know, by others.

17. One of the most ingenious explanations (from the pure contact theory point of view) of the change of potentials near the copper-iron or copper-nickel pair effected by hydrogen sulphide or chloride is that of G. Wiedemann ('Lehre von der Electrikität,' vol. 1, p. 205), who suggests that in addition to the films of sulphide or chloride formed on the metals, a film of hydrogen may be deposited, and the contact effect of all these substances is added to that of the metals. It may be pointed out that precisely the same explanation may be given of the usual experiments made in presence of water-vapour where the oxygen combining with the metal forms an oxide film, which may again be covered with the hydrogen left free.

18. Several other authors have urged as an objection to these experiments, that the action of the gases causes the formation of coatings of various compounds, and so the whole state of things is altered by the introduction of additional bodies or substances assumed to introduce new so-called "contact effects." (See Ayrton and Perry, 'Phil. Mag.,' January, 1881, p. 48; Pellat, 'Journal de Physique,' vol. 10, 1881, or 'Thèses présentées à la Faculté des Sciences de Paris,' No. 461, p. 16.)

Dr. Lodge also states ('Brit. Assoc. Report,' 1884, p. 50), "but satisfactory observation in these gases is difficult, because they not only *tend* to attack the plates, but they *do* attack them, and so a film is formed and everything is rendered uncertain."

But it is surely too much to say that no such film (of oxide) is formed in the experiments as usually made in air. We are even justified in inferring that such a thing as a "contact" experiment on clean metals has not yet been made, since they are invariably cleaned while exposed to the atmosphere, and therefore covered with a layer or film of highly condensed water-vapour containing other dissolved gases.

Each abrading point of the polishing material (emery, glass-paper, or whatever it be) may almost be said to be working under water on the metal.

It seems difficult to suppose that even for an instant after each little abrading point has passed, the surface of the metal exposed in its track remains unoxidised. All such critics assume quite unwarrantably that in the old contact experiments of Volta the surface of the plates is not altered by the atmosphere, whereas the alteration is not only after a time visible to the naked eye, but is accompanied by a sequence of electrical effects in a quite similar way to that when other gases are used, which attack chemically one or other of the metals, so as to mask or overpower the action of the oxidising atmosphere still present. On the other hand, when gases are added

that have not an action differing from that of the air (still present to a greater or less degree), or are merely neutral, there is no great difference in the electrical effects produced (Pellat, 'Thèses présentées à la Faculté des Sciences de Paris,' p. 109; Schulze-Berge, 'Wiedemann, Annalen,' vol. 12, 1881, p. 293). The effect of a hydrogen atmosphere in gradually decreasing the negative potential near platinum is an exception, and has been already well explained by the hypothesis that it forms an alloy with the platinum of a more positive character; or which, according to the chemical theory, has a greater affinity for the oxygen still present than the platinum has.

19. Many attempts have been made to remove completely from the metals under examination all oxidising or other chemically active substances, and thus show that the metals *per se* have no power of producing a difference of potential.

Some of the older writers claim to have effected this by thickly varnishing the plates of the copper-zinc Volta condenser. I doubt if even a thick coat of varnish, which is more or less pervious to gaseous matter, would so effectually protect the surfaces as to leave no effect discernible by the more sensitive modern appliances. Besides, the varnish itself is objected to as a new element in the chain of "contact" effects. Some experiments on the effect of varnish were made with the large Volta condenser shown at fig. 3.

20. This instrument was in general form copied from that described by Schulze-Berge ('Wiedemann, Annalen,' vol. 12, 1881, p. 294). Referring to fig. 3, A is a triangular piece of mahogany 1 inch thick, turning on pointed screws at B, and resting at its other end on a support M. It carries on its lower surface three vulcanite fittings, DD, supporting the copper plate C, which can thus be raised from or lowered towards the zinc plate Z, lying on three vulcanite levelling screws, SS. The plates were 8 inches diameter, and about $\frac{1}{4}$ inch thick.

A measurement of the difference of potential of the films on these plates was made by a method similar to that employed by Schulze-Berge and shown at fig. 4, where D is a Daniell's cell, of small internal resistance, connected to RR′, a set of resistance coils, of which the part a is 200 ohms and x is variable. The key K breaks the connexion of both the condenser plates Cu, Zn at once. In making an experiment the resistance x was adjusted till on breaking the contacts at K, and immediately afterwards separating the plates, no sensible deflection was observed at the quadrant electrometer E in connexion with them. Then the amount of resistance in x divided by the whole resistance RR′ gave in terms of the Daniell the difference of potential required to neutralise that of the films.

21. The zinc plate of this condenser was carefully varnished all over while hot with repeated coats of "silver lacquer" (shell-lac and other gums dissolved in spirits of wine), but the difference of

potential between the varnish on the zinc and the film on the opposed copper plate did not fall below 0·29D taken while the zinc was hot.

On cooling for 15 minutes the value rose to 0·35D, and after some hours to 0·46D, an effect easily caused by the passage of moisture into the varnish. Fifteen hours after it had fallen to 0·4D, indicating probably the formation of a partially protective coat of oxide under the varnish.

The commonly accepted value for plates of these metals recently cleaned in air is about 0·8D.

22. I have also endeavoured to obtain this result, although without much hope of success, by immersing the platinum-zinc quadrants of the electrometer, fig. 1, in naphtha which had been digested with metallic sodium for several months in order to free it if possible from oxidising matters. Metallic sodium was also placed on the bottom of the electrometer. The naphtha was contained in a glass jar, and connected by a syphon to the stopcock S. The difference of potential of the polished metals measured in air was 0·88 D. The naphtha was now run in for five minutes, when its level was about 0·05 inch over the surface of the quadrants, and about the same distance below the needle. The following observations were then made; t denoting time elapsed after the metals had become covered:—

t=8 m.	40 m.	1 h. 40 m.	2 h. 40 m.	4 h.	7 h.	12 h.	22 h.
0·66 D	0·61D	0·49D	0·5D	0·49D.	0·39D.	0·39D	0·31D.

Two hours after this last observation the naphtha was partially run out of the instrument, so as to bring its surface below the level of the quadrants, when a measurement gave only 0·23D. It is difficult to see why the thin film of naphtha still present on the plates should possess a more protective power than the bulk of the liquid.

It may be that there was in the naphtha some small quantity of water or other substance chemically active towards the zinc (the deterioration of sodium or potassium under naphtha is well known) which would soon become exhausted from the thin film, but the hypothesis is open to doubt.

23. Faraday considered that electrolytic "decomposition and the transference of a current are so intimately connected that one cannot happen without the other" ('Exp. Res.,' vol. 1, p. 252). I think it in the highest degree probable that the same theory applies to the production of the momentary current which gives rise to a difference of potential near the surfaces of two apparently dry metals when placed in contact, and that the electrolyte then active consists of the water supplied from the air and condensed in invisible films on them. Hence the chemical action producing the electrical effect is not only

the mere oxidation by free oxygen, but is associated with the decomposition of the condensed film on the metal.

24. The apparatus, fig. 1, was admittedly not sufficiently impervious to gaseous diffusion to permit of a crucial experiment on the effect of drying the atmosphere surrounding the metals. However, in the hope of partial success, the following experiment was begun in 1884. The copper and zinc quadrants were adjusted in position, the water vessel having been removed, and a weight with paper vanes substituted. The difference of potential was found 0·68D. Two small porcelain capsules containing phosphoric anhydride were placed inside the instrument, and the joint between the glass front and the metal facing made as good as possible with grease. The following measurements were then made in terms of standard Daniell; t in this case standing for days after closing up the instrument.

$t=0$	1	2	3	5	7	9	11	13
P/D=0·64	0·62	0·64	0·61	0·59	0·59	0·58	0·58	0·57

$t=20$	35	78	124	134	304	305
P/D= 0·56	0·56	0·557	0·54	0·50	0·52	0·51

The phosphoric anhydride was now taken out, and the instrument allowed to remain open for twenty minutes, after which a very carefully made and satisfactory observation showed that the difference of potential had increased to 0·646D. Fresh phosphoric anhydride was now placed in the instrument, which was closed as before, and the value measured immediately afterwards found to be 0·659D.

A similar series of observations was then made for a period of 173 days, during which the observed difference of potential fell more or less regularly to about 0·5D. Then on allowing the instrument to remain open for one hour it rose to about 0·67D. These results were quite as marked as I had hoped for with such unsuitable apparatus, and seem to show very clearly that even a partial drying of the metallic surfaces alters the difference of potential near them in a decided manner.

25. In 1881 I had endeavoured to construct a copper-zinc quadrant electrometer hermetically sealed in a glass vessel, from which I could exhaust the air and absorb the oxygen compounds by potassium. Repeated failures in the construction of the apparatus have hitherto debarred the experiment. A similar idea occurred to Herr Von Zahn about the same time ('Untersuchungen über Contact-Elektricität,' p. 48).

He enclosed a platinum-zinc condenser in a hermetically sealed tube, together with metallic sodium. The difference of potential was reduced in this case to about 0·5D, which the author attributes, in part at least, to the absence of moisture. He considers the experiment

would be a crucial one in favour of the chemical theory, if, after opening the tube, the difference of potential was found to have increased. He does not, however, open it.

There are two objections to this particular experiment. The tube, &c., was washed out with water which tarnished the zinc. The action of the sodium on moisture would evolve hydrogen, which would alloy with the platinum.

The former would reduce the difference of potential permanently, and the latter perhaps temporarily, but neither would, I think, much affect the conclusion to be drawn from a measurement made immediately on opening the tube.

26. Von Zahn also experimented qualitatively on the difference of potential between a copper plate and a flat spiral of glass tube containing sodium used as a Volta condenser plate, but does not seem to have tested the effect after the lapse of any considerable time, although he states that the surface of the sodium remained bright after standing some years.

If Bunsen's hypothesis ('Phil. Mag.,' vol. 17, 1884, p. 172) that glass may gradually, year after year, absorb a part of the liquid film in contact with it, be true, it would probably be necessary to allow such apparatus to stand a long time before the absorbed water had freed itself from the glass. The same criticism applies to Von Zahn's experiment with a sodium copper condenser *in vacuo*, where the tube broke after the first observation.

27. The foregoing observations, the fact that gases under ordinary conditions are non-conductors, and the much greater simplicity of the theory, all point to the view that the so-called "contact" effects are due to the action of condensed films on the surface of the metals. When the copper and zinc quadrants are wet with water, the difference of potential near them is practically that of the terminals of a cell formed of the same metals dipping in water. If instead of the water we use copper sulphate saturated solution on the copper quadrant and zinc sulphate solution on the zinc, the difference of potential is almost exactly that of the Daniell cell, of which this arrangement is the analogue. In fact whether we break such simple galvanic circuits in their metallic parts, or in their liquid parts, we obtain the same value at the terminals in either case.

28. Considering then the two metals of a copper-zinc Volta condenser as the elements of a cell, and the moisture film on them as the electrolyte, we have a simple copper-water-zinc cell, divided on its electrolyte, and showing, when the metals are clean, approximately the difference of potential appropriate to such a cell.

The well-known experiment of Sir William Thomson, where a water-drop was placed between the previously disconnected metal quadrants, is thus easily explained. The water-drop connexion

equalised (sensibly) the potentials of the films, by altering those of the metals which were previously at one potential.*

29. Evidently then, as I have before suggested ('Phil. Mag.,' Feb., 1879), in experiments purporting to give the difference of potential between a metal and a liquid by the condenser method, such as those of Hankel, Gerland, Clifton, Ayrton and Perry, &c., we have really a two-fluid cell (one fluid being that under examination and the other that condensed on the metal plate), with a dielectric division of air between the two fluids. Any observed differences of potential can readily arise from differences between the nature and constitution of the film and those of the liquid under examination, even if the latter be water. The film can, for instance, more easily replace by absorption from the air any oxygen in solution which may have entered into combination with nascent hydrogen liberated by the oxidation of the metal.

These differences naturally cause different actions on the metal, entailing different states of its surface, which again react on the electrolytes differently.

If in experiments of this kind the metal dipping in the liquid be different to that of which the plate is formed, we have of course then a two-metal two-fluid cell.

30. This aspect of the Volta condenser as a copper-fluid-zinc cell divided in its electrolyte, suggested the possibility of joining the films only on the two metals, without bringing the metals themselves in contact, and so producing a real galvanic current-producing cell from the apparently dry metals. This I succeeded in doing after a very great expenditure of time and patience; an expenditure partly owing to the unsuitability of the apparatus—the condenser, fig. 3, described § 20, having been designed for another purpose, was not capable easily of sufficiently fine adjustment. After the first few preliminary trials had given promise of a decided result, a micrometer screw was added, instead of the support at M, in order to be able to form an approximate idea of the distance between the plates during an experiment.

The copper and zinc plates of the condenser were first carefully faced up to a plane surface by the use of a surface plate, and then lightly ground together with washed emery-powder, after which the zinc received a light rubbing with fine emery-paper. Wire connexions were fixed by screws to each plate, and the pair could thus be joined by means of a mercury cup arrangement either to a reflecting galvanometer, giving a deflection of about seventeen divisions for a millionth

* [When the quadrants of copper and iron were disconnected and then joined by a drop of potassium sulphide solution, the difference of potential near them was (as would be expected) increased instead of annulled. It became about double its former value.—November 1, 1886.]

ampère current, or to a circuit consisting of a Leclanché cell and an ordinary astatic galvanoscope. A telephone was also generally included in the circuits. When joined up with the cell in circuit it was possible by very careful adjustment of the distance between the plates to find a point where although they were not in metallic contact still a current would pass. The needles of the galvanoscope (which flew to the stops when the plates were in actual contact) were deflected unsteadily to about the same extent as if the condenser were replaced by a resistance of 50 to 100 ohms, and a faint hissing sound was heard in the telephone. If now the terminals from the plates were transferred to the galvanometer, a current was observed varying in different experiments according to circumstances from a few up to 130 divisions, but which at once ceased when the plates were either placed in metallic contact or so far apart as to separate the films on them.

31. Measurements of the thickness of the two films were made by the micrometer screw at M. Its indications are, however, only approximate, since the upper plate does not remain parallel to the lower while being raised; and also (the whole apparatus not being very rigid) it is quite possible that when the upper film rested on the lower one, there was sufficient pressure on the surface of the plates to prevent the upper one falling to the extent indicated by the micrometer readings, although a weight of six pounds was placed on the end of the upper board at A to help to depress it. I give, however, a series of readings typical of those obtained in all the experiments.

M denotes the reading on micrometer corresponding to the distance through which it rose in thousandths of an inch. D is the deflection on galvanometer. (The adjustment for great sensitiveness rendered its zero unstable.)

Hygrometric state of air measured by Regnault's form of hygrometer was 0·77.

M=	0·5	0·7	1	1·5	2	2·5	3	3·5	4	4·5	4
D=	10	90	70	90	70	55	40	40	25	10	10

Plates in contact. Films apart.

M=	3·5	3	2·5	2	1·5	1	0·5	0
D=	35	38	40	50	50	50	15	5

Plates in contact.

Hygrometric state of air=0·655—

M=	0	1	1·5	2	2·5	3	3·5	3	2·5	2	1·5
D=	30	65	68	75	65	40	32	32	58	67	35

Plates in contact. Films apart.

The plates were very carefully dusted before use with a camel-hair brush, and it was necessary also to avoid breathing on them, as

the slightest puff of breath over them caused the formation of a film, which, though quite invisible, easily formed enough current to send the index quite off the scale.

32. I now made some experiments to ascertain whether the film cell could be polarised by the current from a Leclanché element connected to the plates.

The following examples show that this is so, and that the polarisation effect is so great as at first even to overpower the ordinary electromotive force of the film cell.

Column I gives the mode of connexion of the poles of the cell to the plates; P the carbon pole.

II.—The deflection observed on the galvanoscope.

III.—The zero point of scale of reflecting galvanometer.

IV.—Its initial swing on having the terminals of film cell changed to it.

V.—The point at which the index rested after its first swing.

(1.) Hygrometric state of air=0·8—

I	II	III	IV	V	
P to Cu.....	25	17	..	30	Falling.
P to Zn.....	71	20	..	16	Rising.
		Readjusted plates.			
P to Cu.....	12	30	..	45	Falling.
P to Zn.....	68	32	..	22	Rising beyond 32.
		Readjusted plates.			
P to Cu.....	16	40	60	47	Falling.
P to Zn.....	74	44	31	40	Rising.

(2.) Hygrometric state of air=0·826—

I.	II.	III.	IV.	V.	
P to Cu.....	52	12	60	30	Falling.
P to Zn.....	74	21	14	..	Rising till after some minutes the deflection passed the zero, showing the ordinary current from film cell.

[32*a*. Several of the foregoing experiments go to show that a cessation of chemical action at the surface of the plates implies a subse-

quent disappearance of the difference of potential near them, and a decrease of the former produces a subsequent decrease of the latter.

In the case where the decrease of chemical action is due to drying, it is, perhaps, sufficiently in correspondence with the theory to suppose that when the film is partially removed, the effect it produces should also partially disappear, although we may not be in a position to specify the particular nature of the molecular process involved, or whether there is included with it anything of the kind described next following.

In the case, however, of the hindrance of chemical action by the formation of an intervening oxide or other compound layer, there appears at first sight a difficulty, since it may be said that after the separation and disposal of the two opposite charges on the two films resident on the metals in contact as above described, the mere formation of intervening oxide on other layers would tend more to keep these charges apart than to allow them to combine.

It must, however, be remembered that in this case the films themselves are also in contact, and that a conducting connexion between them may reasonably be supposed to provide a continuous leak, by which the difference of potential suffers a constant reduction below the normal, and would, therefore, disappear altogether, if the leak were not greatly overpowered by continuous electrolytic combination at the surface of the metal.

The lateral resistance of the film must of course be very great, in order that so much difference of potential may be observable; but it is obvious that unless the film were of infinite conductivity some difference would still exist.

Where the metals are not in immediate contact the films on (or moisture in the pores of) the supports may readily provide the necessary electrolytic connexion, considering the very small quantities of electricity involved. I have observed that when the vulcanite piece, M, fig. 1, carrying the quadrants, became damp from remaining some days inclosed with the water in the vessel below it, the potential difference was considerably decreased.

The existence of "local action" or local currents in the film would seem also not improbable, and would account for some of the phenomena connected with tarnishing, otherwise difficult to explain.—November 1, 1886.]

33. I may here refer to an experiment made prior to these last but related to them. It was based on the supposition that the molecules of water in the condensed film on a metal must (in order to oxidise it) have their oxygen atoms turned and attracted towards it, but if we could reverse this arrangement by attracting the hydrogen atoms, we might be able to retard the oxidising action. Possibly the fact that the potential for a charged conductor is constant throughout

negatives this view. It was thought, however, the results might throw light on questions as to the molecular nature of any electrochemical polarisation of the condensed films as a cause of the static voltaic effect, and for that reason they are here shortly stated. The condenser, fig. 3, was provided with a pair of zinc plates faced up together. The difference of potential of their films after metallic contact of the plates was tested qualitatively by a quadrant electrometer in the usual way. A very small value was found, due doubtless to some slight difference in the state of their surfaces. The plates were then separated to a small distance, and joined to the opposite poles of a battery of 100 Daniell cells. After $17\frac{1}{2}$ days the battery was removed, and the difference of potential of the films on the plates again tested. It was supposed that if any greater oxidation of the positively electrified plate, as compared with the other, was caused by polarisation of the films, there would be a greater difference of potential than before; the film on the tarnished plate being negative. If anything, the reverse was the case, but the effect was very slight, and may have been due to some kind of storage cell action in the supports of the plates. A second trial with the Daniell battery joined the reverse way gave a like negative result. Probably the effect suggested does not take place, or it is masked by local actions in the film.

34. An old experiment of Gassiot ('Phil. Mag.,' vol. 25, 1844, p. 283), where electrical disturbance is produced by altering the capacity of a Volta condenser without contact between its copper and zinc plates, which are merely connected to the gold disks of an electroscope, is assumed by some authors (Von Zahn, 'Untersuchungen über Contactelectricität,' p. 55; Wiedemann, 'Lehre von der Elektricität,' vol. 2, p. 988) to be difficult or incapable of explanation by the contact theory, though I think wrongly so, as there is contact of dissimilar metals, viz., copper-gold and zinc-gold on the connexions to the gold disks of the electroscope used. (Von Zahn's inability to repeat the experiment so as to get Gassiot's result seems to me unaccountable.)

35. In order to exclude any uncompensated contact of dissimilar metals, as in Gassiot's experiment, I modified it by joining the copper plate of the condenser, fig. 3, to the copper quadrant in the electrometer, fig. 1, and the zinc plate to the zinc quadrant. The latter connexion was made by a copper wire, but the dissimilar contacts thereby introduced compensate one another.

On altering the capacity of the condenser a very decided alteration of the potentials near the quadrants was observed. The following table gives the results:—Column I gives the sign of the electrification of the needle. II, the position of the index with condenser plates apart. III, the point to which the index swings on closing them to

within about 0·01 inch. IV, the point where index came to rest with the plates so closed. V, the swing on separating the plates. VI, the final deflection with plates apart. The prefix L signifies a left-hand deflection towards the copper quadrant; R a right-hand one towards the zinc quadrant.

I.	II.	III.	IV.	V.	VI.
+	L 44	L 29	L 38·5	L 55	L 43·5
−	R 55	R 39·5	R 48·5	R 66	R 54
−	R 54	R 38	R 48	R 63·5	R 53·5
+	L 44	L 33	L 39	L 51·5	L 43·5

It is evident there were electrical charges present, although there was no actual metallic contact at which any "Scheidungskraft" could exist to produce them. (See Appendix.)

36. In conclusion, the following diagrams represent the distribution of potential according to the theory here adopted.

Fig. 5 gives the state of things in a closed zinc-water-copper cell of uniform resistance throughout its circuit; N and P understood as joined in closed circuit.

Fig. 6 shows the same cell with the metals apart; the two metals being covered with moisture films, which are practically a portion of the electrolyte and (in so far) at the same potential. This shows also graphically the explanation of Sir William Thomson's "water-drop" experiment, referred to before (§ 28).

Fig. 7 is a cell divided in the copper part of the circuit, where instead of the potential of a film on the zinc, as in fig. 6, we have the lower potential of that on the copper piece connected to the zinc. The terminals of like metal in this way permit the electromotive force of the cell itself to be measured in the usual electrometric way.

Fig. 8 represents either a zinc-water-copper cell divided in its electrolyte, or the ordinary static experiment where the difference of potential of the films on two metals in contact is measured.

It is to be borne in mind that these diagrams are only of a qualitative nature, as the difference of potential between the dry metals and water, &c., in contact with them has not yet been experimentally determined.

Appendix.

The experiment described, § 35, leads to the following theoretical considerations which have been in part suggested and put into form by Mr. J. Larmor, of St. John's College, Cambridge.

37. The zinc sector of the electrometer and the zinc plate of the condenser in connexion with it form an insulated conductor of zinc; the copper sector and the copper plate of the condenser form an insulated conductor of copper. Consider first the hypothesis which asserts that after recent metallic connexion, the potential of the zinc exceeds that of the copper by a definite amount. The plates being primarily at a distance, make them approach each other gradually. As they do so the experiment shows that the difference of potential between them gradually diminishes. Their behaviour is just that of an ordinary charged condenser; as the plates come closer together their capacity is increased, being always inversely proportional to their distance, and therefore for a given charge the difference of potentials is diminished, being always directly proportional to their distance. By bringing the plates very close together their difference of potential may be reduced indefinitely. Having brought them within a very minute distance of each other, establish metallic connexion between them. According to the hypothesis this restores them to their original difference of potential. This is done by the separation of a quantity of electricity equal to the difference of potential multiplied by the capacity of the condenser; the two plates acquire charges of this amount but of opposite signs. This operation involves a gain of energy to the system of amount equal to half the capacity of the condenser multiplied by the square of the difference of potentials. The plates being very close together these quantities are of considerable magnitude. Now the operation of making metallic contact was merely directive and involved no supply of energy from without. This supply must therefore come from an absorption of the heat of the system, or from chemical combination either partial or complete. The first alternative is inadmissible, as it would lead to a perpetual motion; we are, therefore, driven to that of chemical combination, which may either be an action between the zinc and copper at the point where the metallic connexion is made, or an action between one or both of the metals and the surrounding gaseous medium. In either case its amount, though very minute, is not infinitesimal. As there is no evidence of any action between clean zinc and clean copper at ordinary temperatures which involves any liberation of energy worth considering, it seems nearly certain that the actual liberation of energy is due to the other cause, which in the atmosphere is oxidation whether nascent or complete.

The plates with their acquired charges may now be separated by mechanical energy, and their charges imparted to an electric receiver by contact. The plates being now as at first the same cycle may be repeated indefinitely, and a permanent supply of electricity may be thus theoretically obtained at the expense of continued oxidation of the plates in air.

These considerations apply very much in fact to Sir W. Thomson's gravity battery with copper filings and zinc funnel.

38. But now suppose that any atmosphere capable of acting chemically on the plates has been removed. On bringing them close together and making metallic connexion between them, they will not be able to return to their original difference of potential, for the energy required to effect the electrical charge is no longer forthcoming. The facts thus far encourage us therefore to hold the view that the difference of potentials is not an intrinsic property of zinc and copper in themselves, but is the result and manifestation of the fact that their contact in the atmosphere has started a chemical action which has proceeded till choked off by its own results.

According to the ideas of the theory of chemical equilibrium of Clausius, Gibbs, and Helmholtz, it has proceeded until the accumulation of its products has secured that further progress shall not any longer lead to a dissipation of energy.

39. If this argument which refers the difference of potential to an initial chemical action on the surface of the plates where they are in contact with the atmosphere or to a condensed liquid surface film derived from it, be allowed, the next step would be the assignment of some mode of action which would account for the result. And here the idea of a double electrical layer, which has been elaborated by Helmholtz from the phenomena of electrolysis, and so successfully applied by him in explanation of the polarisation of voltaic cells, comes up hopefully.

The oxygen of the moisture in the atmosphere will under proper circumstances combine chemically with the zinc, giving rise to electrical effects, as in Sir W. Thomson's gravity battery already mentioned. When the circumstances are such that actual combination is not possible, the two substances will feel each other's presence, and take up along their surface of contact a conformation of molecular equilibrium; we have a right to assume that this configuration will present a sheet of positively charged atoms, facing an equal sheet of negatively charged atoms, and thus forming a double layer. A precise explanation of how this comes about could hardly be required of us, for so long as we continue to apply to such questions our ordinary notions of electrical attractions at all, there is no way open for explaining the dynamical fact of a difference of potential between two substances in contact, except the assumption of such a layer along the surface of contact. How it is formed may be at any rate illustrated from the phenomena of voltaic polarisation.

40. We may then provisionally contemplate a zinc plate as surrounded by such a double layer, negative inside and positive outside; and a copper plate as surrounded by a similar layer of smaller moment, the moment being measured in the same way as for magnetic

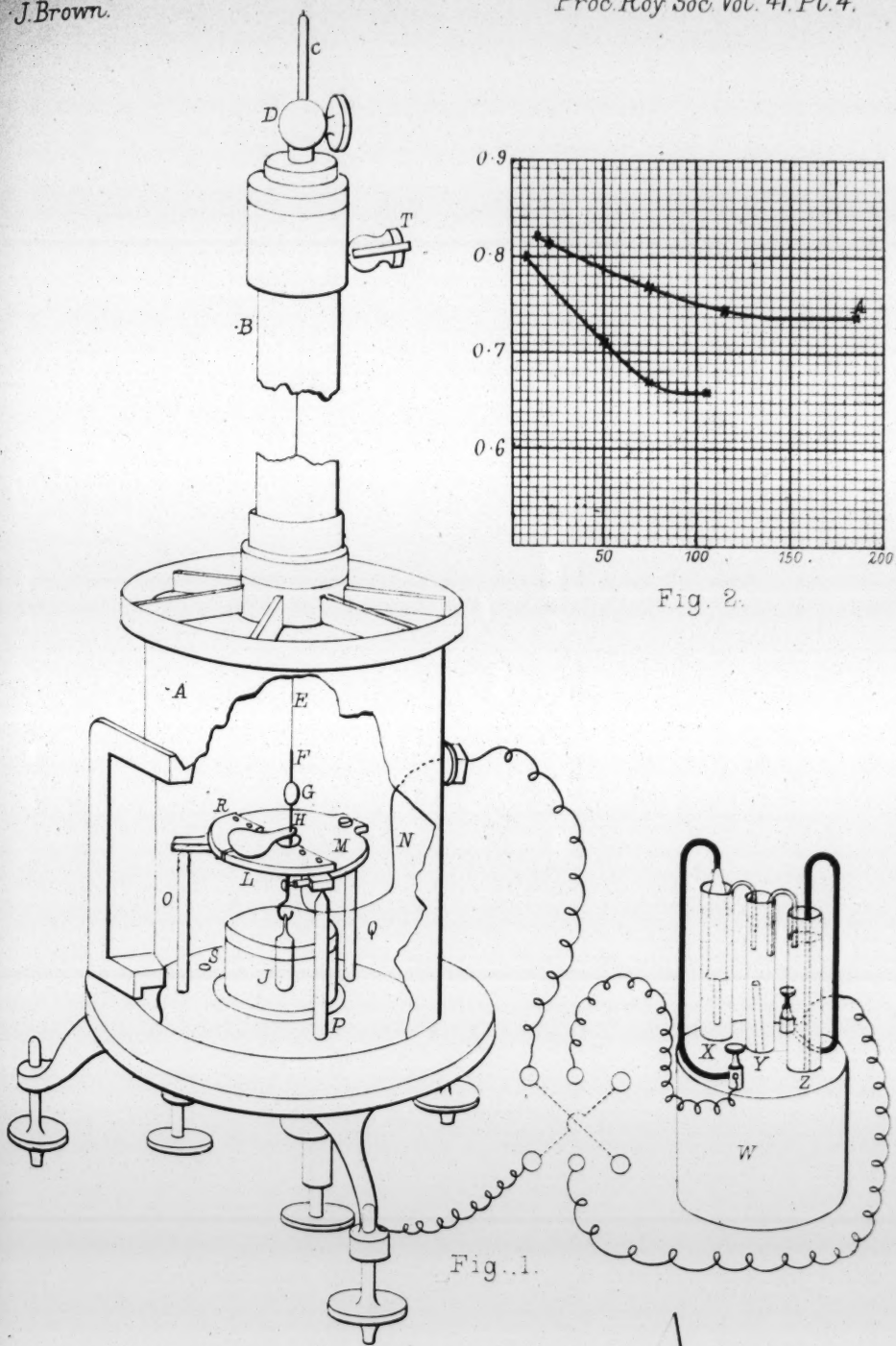


Fig. 1.

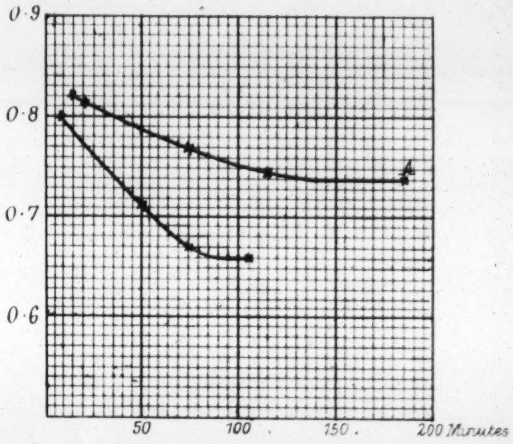


Fig. 2.

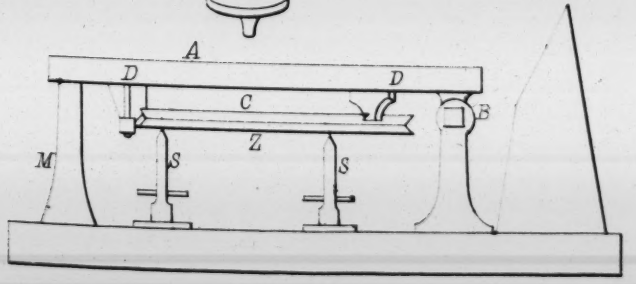


Fig. 3.

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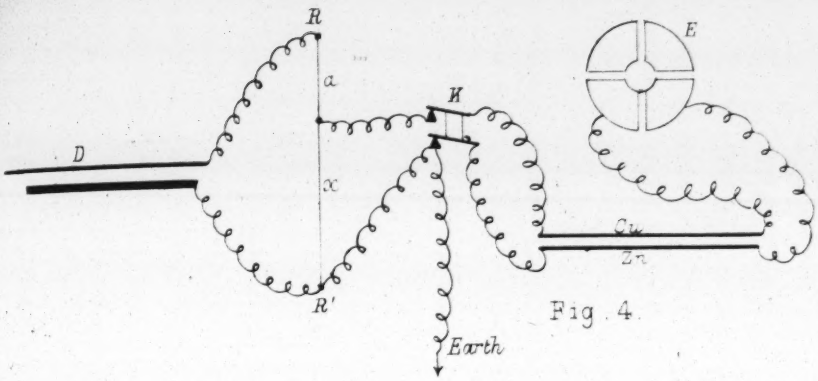


Fig. 4.

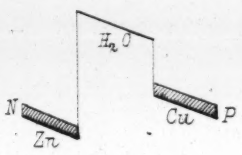


Fig. 5.

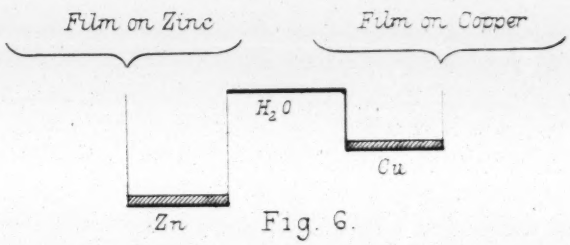


Fig. 6.

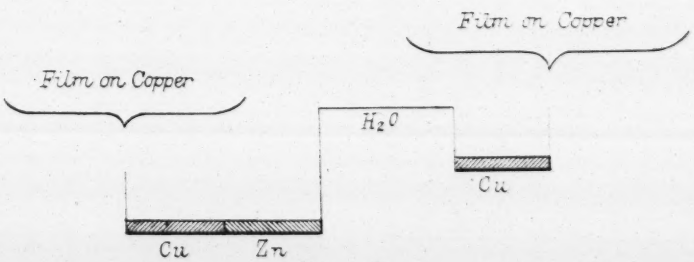


Fig. 7.

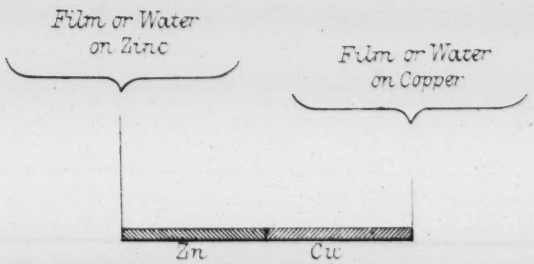


Fig. 8.

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shells, by surface density multiplied by distance between the component layers. When the two plates have attained electrical equilibrium with surrounding objects, the potentials just outside the layers will be equal (*e.g.*, by electrolytic connexion as in Sir W. Thomson's experiment of a zinc and a copper plate connected by a drop of water), but the potentials of the metals inside will be different, each being less by the moment of its layer. The zinc will therefore be at a lower potential than the copper. If a metallic connexion is now made between the metals, positive electricity will flow from the copper to the zinc until the potentials of the metals are equalised, and the difference of potentials of the air outside the plates will now be exactly opposite to that which subsisted between the metals before metallic contact.

There is this difference between the view just mentioned and that of pure contact difference which was examined, § 37, that here the metallic connexion involves a running down of electric energy, while there it involved an addition of energy from without.

41. The explanation by superficial double layers once accepted, there is no longer any reason for retaining contact of the two metals as one of the conditions necessary for the setting up of this action between a metal and its surrounding atmosphere.

We may hold that when a piece of clean zinc, or a piece of clean copper, kept insulated, is introduced into an atmosphere capable under favourable circumstances of acting chemically upon it, a change of a chemical or semi-chemical character is thereby initiated over its surface, which involves a transformation of energy, though in minute quantity, and which proceeds until a state of equilibrium is established, as indicated by the setting up of a certain definite difference of electrical potential between the metal and the gas near it, but outside the sphere of the action.

VI. "The Coefficient of Viscosity of Air. Appendix." By Herbert Tomlinson, B.A. Communicated by Professor G. G. Stokes, P.R.S. Received November 15, 1886.

(Abstract.)

In the previous experiments by the author on this subject the coefficient of viscosity of air was determined from observations of the logarithmic decrement of amplitude of a torsionally vibrating wire, the lower extremity of which was soldered to the centre of a horizontal bar. From the bar were suspended vertically and at equal distances from the wire a pair of cylinders, or a pair of spheres. The distances of the cylinders or spheres from the wire were such that the

main part of the loss of energy resulting from the friction of the air may be characterised as being due to the *pushing* of the air. A small part, however, of the whole logarithmic decrement was due to the rotation of the spheres or cylinders about their axes, and Professor Stokes has kindly added to the paper a note in which are deduced formulæ which serve to correct for this effect of rotation.

Acting on a suggestion of Professor Stokes, the author proceeded to determine the coefficient of viscosity of air by suspending a hollow paper cylinder about 2 feet in length and $\frac{1}{2}$ foot in diameter, so that its axis should coincide as to its direction with the axis of rotation. The cylinder was supported by a light hollow horizontal bar, about 7 inches in length, to the centre of which the vertically suspended wire was soldered. The wire was set in torsional vibration, and the logarithmic decrement determined with the same precautions as before.

The mode of eliminating the effect of the internal friction of the metal wire, and also the effect of the air on the ends of the cylinder, is fully described in the paper.

The following were the results:—

Vibration-period in seconds.	Coefficient of viscosity of air, μ.	Temperature in degrees Centigrade.
3·6038	0·00017708	12·225
8·8656	0·00017783	13·075

In these experiments the loss of energy arising from the friction of the air may be characterised as being due to the *dragging* of the air, and it is very remarkable that there should be such close agreement in the values of μ as determined by this and the previous methods. The mean value of the coefficient of viscosity of air obtained by this method is 0·00017746 at a temperature of 12·650° C., and the mean value deduced from the previous experiments when proper correction has been made for the rotation of the spheres and cylinders about their axes is 0·00017711 at a temperature of 11·79° C. The above values of μ are given in C.G.S. units.

The author expresses his great obligation to Professor Stokes for his valuable suggestions and advice during the progress of the investigation.

November 25, 1886.

Professor STOKES, D.C.L., President, in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election, was read as follows:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—{ Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.—Professor Robert B. Clifton, M.A.; Professor George Howard Darwin, M.A., LL.D.; W. T. Thiselton Dyer, M.A.; Professor David Ferrier, M.A., M.D.; Edward Frankland, D.C.L.; Arthur Gamgee, M.D.; Archibald Geikie, LL.D.; Professor Joseph Henry Gilbert, M.A.; John Hopkinson, M.A., D.Sc.; J. Norman Lockyer, F.R.A.S.; Sir Lyon Playfair, K.C.B., LL.D.; Professor Bartholomew Price, M.A.; Professor Pritchard, M.A.; Admiral Sir George Henry Richards, K.C.B.; Professor Arthur Schuster, Ph.D.; Philip Lutley Sclater, M.A.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "Additional Evidence of the Affinities of the Extinct Marsupial Quadruped, *Thylacoleo carnifex*, Ow." By Sir Richard Owen, K.C.B., F.R.S. Received October 5, 1886.

(Abstract.)

The subject of this paper is an entire lower jaw of the large extinct marsupial which the author from previous fragmentary fossils concluded to be part of a carnivorous beast of the size of a lion, the probable prey of which had been the larger herbivorous marsupials, also now extinct. Every additional character in the subject of the present paper confirms the fitness of the genus named *Thylacoleo*.

II. "On the Structure and Life-History of *Entyloma Ranunculi* (Bonorden)." By H. MARSHALL WARD, M.A., F.L.S., Fellow of Christ's College, Cambridge, and Professor of Botany in the Forestry School, Royal Indian Engineering College, Cooper's Hill. Received October 12.

(Abstract.)

The author found plants of *Ranunculus Ficaria*, the leaves of which were all spotted with white patches; the white patches spread from leaf to leaf, and the disease assumed the nature of an epidemic. The rise, progress, and climax of the disease were observed both on isolated plants and in the open country, and the nature of the lesions in the leaves was made out. Evidence was found to support the view that some plants succumb more rapidly; this evidence was tested, and the circumstances to which the differences are due explained.

The white disease-spots contain the extremely delicate mycelium of *Entyloma Ranunculi*, and the resting-spores of this fungus (one of the Ustilagineæ) were observed on it. The mycelium is intercellular, and makes its way in the middle lamella between contiguous cells. The white powder on the outside of the disease-spot consisted of conidia, very like those of some Ascomycetes. The author examined the anatomical connexion between the conidia and the resting-spores, and showed that the conidia really belong to the same mycelium —in other words, the conidia are a second kind of spore of the *Entyloma*.

Even more important is the germination of the conidia. This has not been before observed in any *Entyloma*. The germination was traced step by step, not only on glass slips, but also on the living plant. These infections yielded the result that the germinal hyphæ entered the stomata, and produced a mycelium exactly like that in the disease-spots first investigated; not only so, but the *resting-spores of the Entyloma were produced on this mycelium*, thus placing beyond doubt the connexion of the two spores. It was observed that it required a certain time for the disease to spread: this interval of time is the same as that occupied in infecting plants with the conidia. Moreover, all the symptoms of the disease produced by infection with the conidia were as before.

III. "On Jacobi's Figure of Equilibrium for a Rotating Mass of Fluid." By G. H. DARWIN, M.A., LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge. Received October 12, 1886.

I am not aware that any numerical values have ever been determined for the axes of the ellipsoids, which are figures of equilibrium of a rotating mass of fluid.*

In the following paper the problem is treated from the point of view necessary for reducing the formulæ to a condition for computation, and a table of numerical results is added.

Let a, b, c be the semi-axes of a homogeneous ellipsoid of unit density; let the origin be at the centre and the axes of x, y, z be in the directions a, b, c.

Then if we put—

$$A^2=a^2+u, \qquad B^2=b^2+u, \qquad C^2=c^2+u, \text{ and}$$

$$\Psi=\int_0^\infty \frac{du}{ABC}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

it is known† that the potential of the ellipsoid at an internal point x, y, z is given by—

$$V=\pi abc\left[\Psi+\frac{x^2}{a}\frac{d\Psi}{da}+\frac{y^2}{b}\frac{d\Psi}{db}+\frac{z^2}{c}\frac{d\Psi}{dc}\right]. \quad . \quad . \quad . \quad . \quad (2)$$

Now let us introduce a new notation, and let

* The following list of papers bearing on this subject is principally taken from a report to the British Association, 1882, by W. M. Hicks:—

Jacobi, 'Acad. des Sciences,' 1834; Liouville, 'Journ. École Polytech.,' vol. xiv, p. 289; Ivory, 'Phil. Trans.,' 1838, Pt. I, p. 57; Pontécoulant, 'Syst. du Monde,' vol. ii. The preceding are proofs of the theorem, and in more detail we have:—C. O. Meyer, 'Crelle,' vol. xxiv, p. 44; Liouville, 'Liouville's Journ.,' vol. xvi, p. 241; a remarkable paper by Dirichlet and Dedekind, 'Borchardt's Journ.,' vol. lviii, pp. 181 and 217; Riemann, 'Abh. K. Ges. Wiss. Göttingen,' vol. ix, 1860, p. 3; Brioschi, 'Borchardt's Journ.,' vol. lix, p. 63; Padova, 'Ann. della Sc. Norm. Pisa,' 1868–9 (being Dirichlet and Riemann's work with additions); Greenhill, 'Proc. Camb. Phil. Soc.,' vol. iii, p. 233 and vol. iv, p. 4; Lipschitz, 'Borch. Journ.,' vol. lxxviii, p. 245; Hagen, 'Schlömilch Zeitsch. Math.,' vol. xxiv, p. 104; Betti, 'Ann. di Matem.' vol. x, p. 173 (1881); Thomson and Tait's 'Nat. Phil.' (1883), Part II, §778; a very important paper by Poincaré, 'Acta Mathem.,' 7, 3 and 4 (1885).

† Thomson and Tait's 'Nat. Phil.' (1883) §494, *l*. The form in which the formula is here given is slightly different from that in (8), (11), (15) of §§ 494, *k*, *l*.

$$c=a\cos\gamma, \quad \sin\alpha=\sqrt{\frac{a^2-b^2}{a^2-c^2}}, \quad \text{and } b=a\cos\beta, \qquad (3)$$

so that $\sin\beta=\sin\alpha\sin\gamma$, and $b=a\sqrt{(1-\sin^2\alpha\sin^2\gamma.)}$

Also let $$A^2=u+a^2=\frac{a^2-c^2}{\sin^2\theta}=a^2\frac{\sin^2\gamma}{\sin^2\theta},$$

whence $$B^2=u+b^2=\frac{a^2\sin^2\gamma}{\sin^2\theta}(1-\sin^2\alpha\sin^2\theta),$$

$$C^2=u+c^2=\frac{a^2\sin^2\gamma}{\sin^2\theta}\cos^2\theta, \qquad (4)$$

and $$du=-\frac{2a^2\sin^2\gamma}{\sin^3\theta}\cos\theta\,d\theta,$$

and $$\int_0^\infty du=2a^2\sin^2\gamma\int_0^\gamma\frac{\cos\theta}{\sin^3\theta}d\theta=2a^2\sin^2\gamma\int_0^\gamma\frac{\cos\gamma}{\sin^3\gamma}d\gamma.$$

Lastly, let $$\Delta=\sqrt{(1-\sin^2\alpha\sin^2\gamma)},$$

and in accordance with the usual notation of elliptic integrals let

$$\mathrm{F}=\int_0^\gamma\frac{d\gamma}{\Delta}, \qquad \mathrm{E}=\int_0^\gamma\Delta\,d\gamma. \qquad (5)$$

Then we have the following transformations:—

$$\Psi=\int_0^\infty\frac{du}{ABC}=\frac{2}{a\sin\gamma}\mathrm{F}$$

$$-\frac{d\Psi}{ada}=\int_0^\infty\frac{du}{A^3BC}=\frac{2}{a^3\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta}d\gamma=\frac{2}{a^3\sin^2\alpha\sin^3\gamma}(\mathrm{F}-\mathrm{E})$$

$$-\frac{d\Psi}{bdb}=\int_0^\infty\frac{du}{AB^3C}=\frac{2}{a^3\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta^3}d\gamma \qquad (6)$$

$$-\frac{d\Psi}{cdc}=\int_0^\infty\frac{du}{ABC^3}=\frac{2}{a^3\sin^3\gamma}\int_0^\gamma\frac{\tan^2\gamma}{\Delta}d\gamma$$

It remains to reduce the last two of (6) to elliptic integrals.

If k and k' be the modulus and its complement, the following are known transformations in the theory of elliptic functions, viz.:—

$$\int_0^\gamma\frac{d\gamma}{\Delta^3}=\frac{1}{k'^2}\mathrm{E}-\frac{k^2\sin\gamma\cos\gamma}{k'^2\Delta}, \qquad (7)$$

$$\int_0^\gamma\frac{\tan^2\gamma}{\Delta}d\gamma=\frac{\Delta\tan\gamma}{k'^2}-\frac{1}{k'^2}\mathrm{E}. \qquad (8)$$

Hence $$\int_0^\gamma\frac{\sin^2\gamma}{\Delta^3}d\gamma=\frac{1}{k^2}\int_0^\gamma\frac{1-\Delta^2}{\Delta^3}d\gamma=\frac{1}{k^2k'^2}\mathrm{E}-\frac{\sin\gamma\cos\gamma}{k'^2\Delta}-\frac{1}{k^2}\mathrm{F}. \qquad (9)$$

In the present case $k=\sin\alpha$, $k'=\cos\alpha$, $\Delta=\cos\beta$. Thus (8) and (9) enable us to complete the required transformation to elliptic integrals of (6).

Substituting then from (6) (8) (9) in the expression

$$V=\tfrac{3}{4}m\left\{\Psi+\frac{x^2}{a}\frac{d\Psi}{da}+\frac{y^2}{b}\frac{d\Psi}{db}+\frac{z^2}{c}\frac{d\Psi}{dc}\right\},$$

where $m=\frac{4}{3}\pi abc=\frac{4}{3}\pi a^3\cos\beta\cos\gamma$, we have

$$V\div\tfrac{3}{4}m=-\frac{2}{a\sin\gamma}F+\frac{2}{a^3\sin^3\gamma}\left[\frac{x^2}{\sin^2\alpha}(E-F)+y^2\left(\frac{\sin\gamma\cos\gamma}{\cos^2\alpha\cos\beta}+\frac{F}{\sin^2\alpha}-\frac{E}{\sin^2\alpha\cos^2\alpha}\right)+\frac{z^2}{\cos^2\alpha}(E-\tan\gamma\cos\beta)\right]. \quad (10)$$

Now suppose the ellipsoid to be rotating about the axis of z with an angular velocity ω, and let us choose the axes a, $a\cos\beta$, $a\cos\gamma$, and the angular velocity ω, so that the surface may be a surface of equilibrium.

For this purpose $V+\frac{1}{2}\omega^2(x^2+y^2)=$constant, must be identical with

$$\frac{x^2}{a^2}+\frac{y^2}{a^2\cos^2\beta}+\frac{z^2}{a^2\cos^2\gamma}=1.$$

Now in (10) we have V in the form

$$V=Lx^2+My^2+Nz^2+P, \quad \ldots\ldots \quad (11)$$

whence $a^2(L+\frac{1}{2}\omega^2)=a^2(M+\frac{1}{2}\omega^2)\cos^2\beta=a^2N\cos^2\gamma$.

Hence
$$\left.\begin{aligned} &L-M+N\cos^2\gamma\tan^2\beta=0,\\ &\tfrac{1}{2}\omega^2=N\cos^2\gamma-L,\\ \text{or}\quad &\tfrac{1}{2}\omega^2\sin^2\beta=M\cos^2\beta-L. \end{aligned}\right\} \quad \ldots\ldots \quad (12)$$

There are two kinds of solutions of these equations (12).

First, since

$$\left.\begin{aligned} L=\pi bc\frac{d\Psi}{da}&=-\pi a^3\cos\beta\cos\gamma\cdot\frac{2}{a^3\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta}d\gamma\\ &=-2\pi\frac{\cos\beta\cos\gamma}{\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta}d\gamma,\\ M=\pi ac\frac{d\Psi}{db}&=-\pi a^3\cos\beta\cos\gamma\cdot\frac{2}{a^3\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta^3}d\gamma\\ &=-2\pi\frac{\cos\beta\cos\gamma}{\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta^3}d\gamma, \end{aligned}\right\} \quad (13)$$

it is obvious that $L-M$ vanishes when $\alpha=0$.

And since when α vanishes, β also vanishes, the equation

$$L-M+N\cos^2\gamma\tan^2\beta=0$$

is satisfied by $\alpha=0, \quad \beta=0.$

That is to say there is a solution of the problem which makes $a=b$.

Thus there is solution which gives us an ellipsoid of revolution.

When $\sin\alpha=0$, we have also $\beta=0$, $\Delta=1$, and

$$L=-\frac{2\pi\cos\gamma}{\sin^3\gamma}\int_0^\gamma\sin^2\gamma\,d\gamma=\frac{\pi\cos\gamma}{\sin^3\gamma}(\sin\gamma\cos\gamma-\gamma),$$

$$N=-\frac{2\pi\cos\gamma}{\sin^3\gamma}\int_0^\gamma\tan^2\gamma\,d\gamma=\frac{\pi\cos\gamma}{\sin^3\gamma}(2\gamma-2\tan\gamma).$$

Therefore

$$\tfrac{1}{2}\omega^2=N\cos^2\gamma-L,$$

$$=\frac{\pi}{\tan^3\gamma}[2\gamma-2\tan\gamma-(1+\tan^2\gamma)(\sin\gamma\cos\gamma-\gamma)],$$

$$=\frac{\pi}{\tan^3\gamma}[\gamma(3+\tan^2\gamma)-3\tan\gamma], \quad \ldots\ldots\ldots \quad (14)^*$$

and the eccentricity of the ellipsoid of revolution is $\sin\gamma$.

To find the other solution when α is not zero, we have by comparison between (10) and (11),

$$\left.\begin{aligned} L\,.\,\frac{\sin^2\alpha\sin^3\gamma}{2\pi\cos\beta\cos\gamma}&=\mathrm{E}-\mathrm{F},\\ M\,.\,\frac{\sin^2\alpha\sin^3\gamma}{2\pi\cos\beta\cos\gamma}&=\frac{\sin^2\alpha\sin\gamma\cos\gamma}{\cos^2\alpha\cos\beta}+\mathrm{F}-\mathrm{E}\sec^2\alpha,\\ N\,.\,\frac{\sin^2\alpha\sin^3\gamma}{2\pi\cos\beta\cos\gamma}&=\tan^2\alpha(\mathrm{E}-\tan\gamma\cos\beta). \end{aligned}\right\} \quad (15)$$

Hence the first of (12) gives

$$-(2\mathrm{F}-\mathrm{E})+\mathrm{E}\sec^2\alpha$$
$$+\tan^2\alpha\tan^2\beta\cos^2\gamma(\mathrm{E}-\tan\gamma\cos\beta)-\frac{\sin^2\alpha\sin\gamma\cos\gamma}{\cos^2\alpha\cos\beta}=0,$$

* Compare with Thomson and Tait's 'Nat. Phil.,' § 771, (3); or any other work which gives a solution of the problem.

or

$$\mathrm{E}\sec^2\alpha[1+(\sin\alpha\tan\beta\cos\gamma)^2]-(2\mathrm{F}-\mathrm{E})$$
$$-\sec^2\alpha\sin\alpha\tan\beta\cos\gamma(1+\sin^2\beta)=0. \quad . \quad . \quad . \quad (16)$$

In order to adapt this for computation, we may introduce the auxiliary angles defined by

$$\tan\zeta=\sin\alpha\tan\beta\cos\gamma,\ \tan\delta=\sin\beta, \quad . \quad . \quad . \quad . \quad (17)$$

and the equation becomes

$$\mathrm{E}\sec^2\alpha\sec^2\zeta-(2\mathrm{F}-\mathrm{E})-\sec^2\alpha\sec^2\delta\tan\zeta=0. \quad . \quad . \quad (18)$$

The second of (12) gives

$$\frac{\omega^2}{4\pi}\cdot\frac{\sin^2\alpha\sin^3\gamma}{\cos\beta\cos\gamma}=\tan^2\alpha\cos^2\gamma(\mathrm{E}-\tan\gamma\cos\beta)-(\mathrm{E}-\mathrm{F}),$$

whence
$$\frac{\omega^2}{4\pi\cos\beta\cos\gamma}=\frac{\mathrm{F}-\mathrm{E}}{\sin^2\alpha\sin^3\gamma}+\frac{\mathrm{E}\cos^2\gamma}{\sin^3\gamma\cos^2\alpha}-\frac{\cos\beta\cos\gamma}{\cos^2\alpha\sin^2\gamma},$$

$$\frac{\omega^2}{4\pi}=\cot\beta\operatorname{cosec}\beta\cot\gamma(\mathrm{F}-\mathrm{E})+\cot^3\gamma\cos\beta\sec^2\alpha\mathrm{E}-\cos^2\beta\cot^2\gamma\sec^2\alpha.$$
$$. \quad . \quad . \quad (19)$$

Some of the subsequent computations were, however, actually made from a formula deduced from the third of (12), which leads to

$$\frac{\omega^2}{4\pi}=\cot\beta\cot\gamma\operatorname{cosec}^3\beta(1+\cos^2\beta)(\mathrm{F}-\mathrm{E})-\cot^3\beta\cot\gamma\tan^2\alpha\operatorname{cosec}\beta\mathrm{E}$$
$$+\cot^2\beta\cot^2\gamma\sec^2\alpha. \quad . \quad . \quad (20)$$

By subtracting (20) from (19) we can deduce (16); hence it follows that (19) and (20) lead to identical results. Most of the subsequent results were computed from both (19) and (20), thus verifying the solution of (18).

The formulæ (18) and (19) are suitable for finding the solution, except when α is small or nearly 90°, when the elliptic integrals become awkward to use. I have, therefore, found approximate formulæ for these cases, but as the algebraic process necessary to establish them is somewhat tedious, the details are given in a note.*

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* *Approximate Solutions of the Problem.*

From (7) we have

$$\int_0^\gamma\frac{\sin^4\gamma}{\Delta^3}d\gamma=\frac{1}{k^4}\int_0^\gamma\frac{(1-\Delta^2)^2}{\Delta^3}d\gamma$$
$$=\frac{1}{k^4k'^2}\mathrm{E}-\frac{1}{k^4}(2\mathrm{F}-\mathrm{E})-\frac{\sin\gamma\cos\gamma}{k^2k'^2\Delta}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Approximate Solutions of the Problem—continued.

Now, since (16) may be written

$$\frac{1}{k'^2}\mathrm{E}-(2\mathrm{F}-\mathrm{E})-\frac{k^2\sin\gamma\cos\gamma}{k'^2\Delta}=\frac{k^4}{\Delta^2}\sin^2\gamma\cos^2\gamma\left[-\frac{1}{k'^2}\mathrm{E}+\frac{\Delta}{k'^2}\tan\gamma\right],$$

it follows from (*a*) and (8) that it may be written

$$\Delta^2\int_0^\gamma\frac{\sin^4\gamma}{\Delta^3}d\gamma=\sin^2\gamma\cos^2\gamma\int_0^\gamma\frac{\tan^2\gamma}{\Delta}d\gamma. \quad \ldots\ldots \quad (b)$$

Again, we may write (19) thus:—

$$\begin{aligned}\frac{\omega^2}{4\pi}&=\frac{\Delta\cos\gamma}{k^2\sin^3\gamma}(\mathrm{F}-\mathrm{E})+\frac{\Delta\cos^3\gamma}{k'^2\sin^3\gamma}\mathrm{E}-\frac{\Delta^2\cos^2\gamma}{k'^2\sin^2\gamma}\\&=\frac{\Delta\cos\gamma}{\sin^3\gamma}\int_0^\gamma\frac{\sin^2\gamma}{\Delta}d\gamma-\frac{\Delta\cos^3\gamma}{\sin^3\gamma}\int_0^\gamma\frac{\tan^2\gamma}{\Delta}d\gamma\\&=\frac{\Delta\cos\gamma}{\sin^3\gamma}\left[\int_0^\gamma\frac{\sin^2\gamma}{\Delta}d\gamma-\cos^2\gamma\int_0^\gamma\frac{\tan^2\gamma}{\Delta}d\gamma\right]. \quad \ldots\ldots \quad (c)\end{aligned}$$

It is easier to develop the equations when written in the forms (*b*) and (*c*) than when we work directly from the elliptic integrals.

Write for brevity

$$k=\sin\alpha,\quad g=\cos\alpha,\quad p=\cos\gamma,\quad q=\sin\gamma,\quad \mathrm{Q}=\tan\gamma,\quad \Lambda=\log_e\cot(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma).$$

First, when k is small—

The following definite integrals are required:—

$$\int q^n d\gamma=-\frac{1}{n}q^{n-1}p+\frac{n-1}{n}\int q^{n-2}d\gamma. \quad \ldots\ldots \quad (d)$$

$$\int\frac{q^n}{p^2}d\gamma=\frac{q^{n-1}}{p}-(n-1)\int q^{n-2}d\gamma. \quad \ldots\ldots \quad (e)$$

From (*d*) and from $\frac{1}{\Delta^3}=\frac{1}{(1-k^2q^2)^{\frac{3}{2}}}=1+\frac{3}{2}k^2q^2+\frac{3.5}{2.4}k^4q^4+\ldots$

$$\int\frac{q^4}{\Delta^3}d\gamma=-\frac{1}{4}q^3p-\frac{3}{4.2}qp+\frac{3.1}{4.2}\gamma+\frac{3}{2}k^2\left(-\frac{1}{6}q^5p-\frac{5}{6.4}q^3p-\frac{5.3}{6.4.2}qp+\frac{5.3.1}{6.4.2}\gamma\right)+. \quad (f)$$

$\Delta^2=1-k^2q^2$, and

$$\begin{aligned}\Delta^2\int\frac{q^4}{\Delta^3}d\gamma&=-\frac{1}{4}q^3p-\frac{3}{4.2}qp+\frac{3.1}{4.2}\gamma+k^2\left[-\left(\frac{3.1}{6.2}-\frac{1}{4}\right)q^3p-\left(\frac{3.5}{6.4.2}-\frac{3.1}{4.2}\right)q^3p\right.\\&\qquad\left.-\frac{3.5.3}{2.6.4.2}qp+\gamma\left(\frac{3.5.3.1}{2.6.4.2}-\frac{3.1}{4.2}q^2\right)\right]+\ldots\\&=-\frac{1}{4}q^3p-\frac{3}{8}qp+\frac{3}{8}\gamma+k^2\left[\frac{1}{16}q^3p-\frac{15}{32}qp+\gamma\left(\frac{15}{32}-\frac{3}{8}q^2\right)\right]. \quad \ldots \quad (g)\end{aligned}$$

Again, $\frac{1}{\Delta}=1+\frac{1}{2}k^2q^2+\ldots$, and by (*e*)—

Approximate Solutions of the Problem—continued.

$$\int\frac{Q^2}{\Delta}d\gamma=\int\frac{q^2}{p^2\Delta}d\gamma=\frac{q}{p}-\gamma+\frac{1}{2}k^2\left[\frac{q^3}{p}+\frac{3}{2}qp-\frac{3}{2}\gamma\right]+\ldots$$

$$p^2q^2\int\frac{Q^2}{\Delta}d\gamma=pq^3-p^2q^2\cdot\gamma+\frac{1}{2}k^2\left[pq^5-\frac{3}{2}pq^5+\frac{3}{2}pq^3-\frac{3}{2}\gamma p^2q^2\right]+\ldots$$

$$=pq^3-p^2q^2\cdot\gamma+k^2\left(-\frac{1}{4}pq^5+\frac{3}{4}pq^3-\frac{3}{4}\gamma p^2q^2\right)+\ldots\ldots\ldots\ldots\quad(h)$$

The equation (b) is $p^2q^2\int\frac{Q^2}{\Delta}d\gamma=\Delta^2\int\frac{q^4}{\Delta^3}$, whence, equating (g) and (h),

$$\frac{5}{4}q^3p+\frac{3}{8}pq-\frac{3}{8}\gamma\left(1+\frac{8}{3}p^2q^2\right)+k^2\left[-\tfrac{1}{4}pq^5+\frac{11}{16}pq^3+\frac{15}{32}pq-\gamma\left(\frac{15}{32}+\frac{3}{8}q^2-\frac{3}{4}q^4\right)\right]$$

$$+\ldots=0.$$

If k be zero, we have $\quad\frac{5}{4}q^3p+\frac{3}{8}pq-\gamma\left(\frac{3}{8}+p^2q^2\right)=0.$

This easily reduces to $\quad\gamma=\dfrac{\sin 2\gamma-\frac{5}{16}\sin 4\gamma}{1-\frac{1}{4}\cos 4\gamma},\quad\ldots\ldots\ldots\quad(i)$

of which the solution is $\gamma=54^\circ\ 21'\ 27''$, as stated in the text.

Now

$$\frac{d}{d\gamma}\left[\frac{5}{4}q^3p+\frac{3}{8}pq-\gamma\left(\frac{3}{8}+p^2q^2\right)\right]=-\frac{1}{2}[1-2\cos 2\gamma+\cos 4\gamma+\gamma\sin 4\gamma],$$

and with the above value of γ is equal to $-0{\cdot}1355014$.

Also, with this value of γ the coefficient of k^2 is $0{\cdot}0160432$; so that

$$0{\cdot}0160432\sin(\gamma-54^\circ\ 21'\ 27'')=0{\cdot}1355014k^2,$$

or,
$$\sin^2\alpha=10^{\cdot 9266528}\sin(\gamma-54^\circ\ 21'\ 27''),$$

which is the equation (21) of the text.

Again $\quad\displaystyle\int_0^\gamma\frac{q^2}{\Delta}d\gamma=-\frac{1}{2}qp+\frac{1}{2}\gamma+\frac{1}{2}k^2\left(-\frac{1}{4}q^3p-\frac{3}{8}qp+\frac{3}{8}\gamma\right),$

$$p^2\int\frac{Q^2}{\Delta}d\gamma=qp-(1-q^2)\gamma+\frac{1}{2}k^2\left(q^3p+\frac{3}{2}qp(1-q^2)-\frac{3}{2}\gamma(1-q^2)\right)$$

$$=qp-\gamma+q^2\gamma+\frac{1}{2}k^2\left(-\frac{1}{2}q^3p+\frac{3}{2}qp-\frac{3}{2}\gamma+\frac{3}{2}q^2\gamma\right).$$

Therefore

$$\int\frac{q^2}{\Delta}d\gamma-p^2\int\frac{Q^2}{\Delta}d\gamma=-\frac{3}{2}qp+\left(\frac{3}{2}-q^2\right)\gamma+\frac{1}{2}k^2\left[\frac{1}{4}q^3p-\frac{15}{8}qp+\left(\frac{15}{8}-\frac{3}{2}q^2\right)\gamma\right].$$

Now $\quad\Delta=1-\tfrac{1}{2}k^2q^2-\ldots$

Approximate Solutions of the Problem—continued.

Therefore

$$\Delta\left[\int\frac{q^2}{\Delta}d\gamma-p^2\int\frac{Q^2}{\Delta}d\gamma\right]=-\frac{3}{2}qp+\left(\frac{3}{2}-q^2\right)\gamma$$
$$+\frac{1}{2}k^2\left[\frac{7}{4}q^3p-\frac{15}{8}qp+\left(\frac{15}{8}-3q^2+q^4\right)\gamma\right]$$
$$=p^2\left[-\frac{3}{2}Q+\left(\frac{3}{2}+\frac{1}{2}Q^2\right)\gamma\right]+\frac{1}{2}k^2p^2\left[\frac{7}{4}q^2Q-\frac{15}{8}Q+\left(\frac{15}{8}-\frac{9}{8}Q^2+q^2Q^2\right)\gamma\right]\ldots(j)$$

Hence from (*c*) and (*j*)

$$\frac{\omega^2}{4\pi}=\frac{1}{2Q^3}[(3+Q^2)\gamma-3Q]+\frac{1}{2}\frac{k^2}{Q^3}\left[\frac{7}{4}q^2Q-\frac{15}{8}Q+\left(\frac{15}{8}-\frac{9}{8}Q^2+q^2Q^2\right)\gamma\right],$$

which is the equation (22) of the text.

Secondly, let k be nearly unity and g small—

Then we require the following definite integrals:—

$$\int\frac{q^{n+1}}{p^n}d\gamma=\frac{1}{n-1}qQ^{n-1}-\frac{n}{n-1}\int\frac{q^{n-1}}{p^{n-2}}d\gamma.\quad\ldots\ldots(k)$$

$$\int\frac{q^2}{p}d\gamma=-q+\Lambda.\quad\ldots\ldots\ldots\ldots\ldots(l)$$

Now
$$\Delta^2=1-k^2q^2=p^2(1+g^2q^2p^{-2}).$$
$$\frac{1}{\Delta^3}=\frac{1}{p^3}\left(1-\frac{3}{2}g^2\frac{q^2}{p^2}+\ldots\right).$$
$$\frac{q^4}{\Delta^3}=\frac{q^4}{p^3}-\frac{3}{2}g^2\frac{q^6}{p^5}+\ldots$$

By (*k*) and (*l*)—

$$\int\frac{q^4}{\Delta^3}d\gamma=\frac{1}{2}qQ^2+\frac{3}{2}q-\frac{3}{2}\Lambda-\frac{3}{2}g^2\left[\frac{1}{4}qQ^4-\frac{5}{4.2}qQ^2-\frac{5.3}{4.2^2}q+\frac{5.3}{4.2}\Lambda\right]\ldots$$
$$=\frac{3}{2}q\left[1+\frac{1}{3}Q^2+g^2\left(\frac{15}{8}+\frac{5}{8}Q^2-\frac{1}{4}Q^4\right)\right]-\frac{3}{2}\Lambda\left(1+\frac{15}{8}g^2\right)\ldots$$

Now
$$\frac{\Delta^2}{p^2}=1+g^2Q^2,$$

therefore

$$\frac{\Delta^2}{p^2}\int\frac{q^4}{\Delta^3}d\gamma=\frac{3}{2}q\left[1+\frac{1}{3}Q^2+g^2\left(\frac{15}{8}+\frac{13}{8}Q^2+\frac{1}{12}Q^4\right)\right]-\frac{3}{2}\Lambda\left[1+g^2\left(\frac{15}{8}+Q^2\right)\right]\ldots$$

But
$$\frac{1}{p^2}=1+Q^2,$$

therefore

$$\frac{\Delta^2}{p^4}\int\frac{q^4}{\Delta^3}d\gamma=\frac{3}{2}q\left[1+\frac{4}{3}Q^2+\frac{1}{3}Q^4+g^2\left(\frac{15}{8}+\frac{28}{8}Q^2+\frac{41}{24}Q^4+\frac{1}{12}Q^6\right)\right]$$
$$-\frac{3}{2}\Lambda\left[1+Q^2+g^2\left(\frac{15}{8}+\frac{23}{8}Q^2+Q^4\right)\right].\quad\ldots\ldots\ldots(m)$$

Approximate Solutions of the Problem—continued.

Again,
$$\frac{1}{\Delta}=\frac{1}{p}\left(1-\frac{1}{2}g^2\frac{q^2}{p^2}\cdots\right).$$

$$\frac{Q^2}{\Delta}=\frac{q^4}{p^3}+\frac{q^2}{p}-\frac{1}{2}g^2\left(\frac{q^6}{p^5}+\frac{q^4}{p^3}\right)+\cdots$$

$$\int\frac{Q^2}{\Delta}d\gamma=\left(\frac{1}{2}qQ^2+\frac{3}{2}q-\frac{3}{2}\Lambda\right)+(-q+\Lambda)-\frac{1}{2}g^2\left[\left(\frac{1}{4}qQ^4-\frac{5}{4.2}qQ^2-\frac{5.3}{4.2}q+\frac{5.3}{4.2}\Lambda\right)\right.$$
$$\left.+\left(\frac{1}{2}qQ^2+\frac{3}{2}q-\frac{3}{2}\Lambda\right)\right]\cdots$$

$$=\frac{1}{2}q(1+Q^2)+\frac{1}{2}qg^2\left(\frac{3}{8}+\frac{1}{8}Q^2-\frac{1}{4}Q^4\right)-\frac{1}{2}\Lambda\left(1+\frac{3}{8}g^2\right)\cdots$$

$$Q^2\int\frac{Q^2}{\Delta}d\gamma=\frac{3}{2}q\left[\frac{1}{3}Q^2+\frac{1}{3}Q^4+g^2\left(\frac{1}{8}Q^2+\frac{1}{24}Q^4-\frac{1}{12}Q^6\right)\right]-\frac{3}{2}\Lambda\left(\frac{1}{3}Q^2+\frac{1}{8}g^2Q^2\right). \quad (n)$$

The equation (b) for determining the axes is

$$\frac{\Delta^2}{p^4}\int\frac{q^4}{\Delta^3}d\gamma=Q^2\int\frac{Q^2}{\Delta}d\gamma.$$

Hence equating (m) and (n) and dividing by $\frac{3}{2}q$ we have

$$1+Q^2+g^2\left(\frac{15}{8}+\frac{27}{8}Q^2+\frac{5}{3}Q^4+\frac{1}{6}Q^6\right)-\frac{\Lambda}{q}\left[1+\frac{2}{3}Q^2+g^2\left(\frac{15}{8}+\frac{11}{4}Q^2+Q^4\right)\right]\cdots=0,$$

which is the equation (23) in the text.

The equation (c) for ω may be written

$$\frac{\omega^2}{4\pi}=\frac{\Delta}{Q^3}\left[(1+Q^2)\int\frac{q^2}{\Delta}d\gamma-\int\frac{Q^2}{\Delta}d\gamma\right].$$

Now
$$\int\frac{q^2}{\Delta}d\gamma=\int\left[\frac{q^2}{p}-\frac{1}{2}g^2\left(\frac{q^2}{p}+\frac{q^4}{p^3}\right)\right]d\gamma$$
$$=\left(1-\frac{1}{2}g^2\right)(q-\Lambda)-\frac{1}{2}g^2\left[\frac{1}{2}qQ^2+\frac{3}{2}q-\frac{3}{2}\Lambda\right]$$
$$=q\left\{\frac{\Lambda}{q}-1+\frac{1}{2}g^2\left[\frac{1}{2}\Lambda-\frac{1}{2}-\frac{1}{2}Q^2\right]\right\}.$$

$$(1+Q^2)\int\frac{q^2}{\Delta}d\gamma=q\left\{\left(\frac{\Lambda}{q}-1\right)(1+Q^2)+\frac{1}{2}g^2\left[\frac{1}{2}\Lambda(1+Q^2)-\frac{1}{2}(1+Q^2)^2\right]\right\}$$

$$\int\frac{Q^2}{\Delta}d\gamma=q\left\{\frac{1}{2}(1+Q^2)-\frac{1}{2}\Lambda+\frac{1}{2}g^2\left[\frac{3}{8}+\frac{1}{8}Q^2-\frac{1}{4}Q^4-\frac{3}{8}\Lambda\right]\right\}$$

$$(1+Q^2)\int\frac{q^2}{\Delta}d\gamma-\int\frac{Q^2}{\Delta}d\gamma=q\left\{\frac{\Lambda}{q}\left(\frac{3}{2}+Q^2\right)-\frac{3}{2}(1+Q^2)+\frac{1}{2}g^2\left[\frac{\Lambda}{q}\left(\frac{7}{8}+\frac{1}{2}Q^2\right)-\frac{7}{8}-\frac{9}{8}Q^2\right.\right.$$
$$\left.\left.-\frac{1}{4}Q^4\right]\right\}.$$

If α be infinitely small, so that the Jacobian ellipsoid of three unequal axes becomes in the limit an ellipsoid of revolution, we have γ given by

$$\gamma = \frac{\sin 2\gamma - \frac{5}{16}\sin 4\gamma}{1-\frac{1}{4}\cos 4\gamma}.$$

The solution of this is $\gamma = 54^\circ\ 21'\ 27''$.

If we write $\tan\gamma = f$, as in Thomson and Tait's 'Natural Philosophy,' §778′, this equation becomes

$$\frac{\tan^{-1}f}{f} = \frac{1+\frac{1}{3}f^2}{1+\frac{1}{3}f^2+f^4},$$

which is the equation (9), §778′, of that work.

The ellipsoid of revolution of which the eccentricity is sin 54° 21′ 27″ belongs to the revolutional series of figures of equilibrium, and is the starting point of the Jacobian series of figures. As shown by Sir William Thomson, it is the flattest revolutional figure which is dynamically stable. The Jacobian figures of equilibrium are initially stable, and as stated by M. Poincaré,* there is for this value of γ a crossing point of the two series, and an exchange of stabilities.

If α be small, it appears that $\sin\alpha$ is given by

$$\sin^2\alpha = 10^{\cdot 9266528}\sin(\gamma - 54^\circ\ 21'\ 27''), \quad . \ . \ . \quad (21)$$

and ω by

$$\frac{\omega^2}{4\pi} = \tfrac{1}{2}\cot^3\gamma[(3+\tan^2\gamma)\gamma - 3\tan\gamma]$$
$$+\tfrac{1}{2}\frac{\sin^2\alpha}{\tan^3\gamma}[(\tfrac{7}{4}\sin^2\gamma - \tfrac{15}{8})\tan\gamma + \gamma(\tfrac{5}{8} - (\tfrac{9}{8} - \sin^2\gamma)\tan^2\gamma)] \ . \quad (22)$$

Approximate Solutions of the Problem—continued.

Now $$\Delta = p\left(1+\frac{1}{2}g^2Q^2\ldots\right).$$

Therefore

$$\Delta\left[(1+Q^2)\int\frac{q^2}{\Delta}d\gamma - \int\frac{Q^2}{\Delta}d\gamma\right] = pq\left\{\frac{\Lambda}{q}\left(\frac{3}{2}+Q^2\right) - \frac{3}{2}(1+Q^2)\right.$$
$$\left. +\frac{1}{2}g^2\left[\Lambda\left(\frac{7}{8}+2Q^2+Q^4\right) - \frac{7}{8} - \frac{21}{8}Q^2 - \frac{7}{4}Q^4\right]\right\}.$$

Hence

$$\frac{\omega^2}{4\pi} = \frac{pq}{Q^3}\left\{\frac{\Lambda}{q}\left(\frac{3}{2}+Q^2\right) - \frac{3}{2}(1+Q^2) + \frac{1}{2}g^2\left[\frac{\Lambda}{q}\left(\frac{7}{8}+2Q^2+Q^4\right) - \frac{7}{8} - \frac{21}{8}Q^2 - \frac{7}{4}Q^4\right]\right\},$$

which is the equation (24) in the text.

* "Sur l'Équilibre d'une Masse de Fluide, &c.," 'Acta Mathematica,' 7, 1886.

These formulæ are, it must be admitted, of but little use, since it would be necessary to take in higher powers of $\sin^2\alpha$ to obtain results for a variation of γ of more than 1°.

If α be near 90°, so that $\cos\alpha$ is small, the approximate equation between α and γ is

$$1+\tan^2\gamma+\cos^2\alpha(\tfrac{15}{8}+\tfrac{27}{8}\tan^2\gamma+\tfrac{5}{3}\tan^4\gamma+\tfrac{1}{6}\tan^6\gamma)$$

$$-\frac{1}{\sin\gamma}\log_e\cot(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma)\cdot[1+\tfrac{2}{3}\tan^2\gamma+\cos^2\alpha(\tfrac{15}{8}+\tfrac{11}{4}\tan^2\gamma+\tan^4\gamma)]$$
$$=0. \quad . \quad . \quad . \quad (23)$$

And ω is given by

$$\frac{\omega^2}{4\pi}=\tfrac{1}{2}\frac{\sin 2\gamma}{\tan^3\gamma}\left\{\frac{1}{\sin\gamma}\log_e\cot(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma)\cdot(\tfrac{3}{2}+\tan^2\gamma)-\tfrac{3}{2}\sec^2\gamma\right.$$

$$+\tfrac{1}{2}\cos^2\alpha\left[\frac{1}{\sin\gamma}\log_e\cot(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma)\cdot(\tfrac{7}{8}+2\tan^2\gamma+\tan^4\gamma)-\tfrac{7}{8}-\tfrac{21}{8}\tan^2\gamma\right.$$

$$\left.\left.-\tfrac{7}{4}\tan^4\gamma\right]\right\}. \quad . \quad (24)$$

We shall return later to a modification of (24) which will be applicable to very long ellipsoids of equilibrium.

Besides the angular velocity and the axes of the ellipsoid, the other important functions are the moment of momentum, the kinetic energy of rotation, and the intrinsic energy of the mass. In order to express these numerically we must adopt a unit of length, and it will be convenient to take a, where

$$\mathrm{a}^3 = abc = a^3\cos\beta\cos\gamma.$$

Thus
$$a = \mathrm{a}(\sec\beta\sec\gamma)^{\frac{1}{3}}.$$

Let σ be the density of the fluid which has hitherto been treated as unity, and let $(\frac{4}{3}\pi\sigma)^{\frac{1}{3}}\mathrm{a}^5\mu$, $(\frac{4}{3}\pi\sigma)^2\mathrm{a}^5\epsilon$ be the moment of momentum and kinetic energy, then

$$(\tfrac{4}{3}\pi\sigma)^{\frac{1}{3}}\mathrm{a}^5\mu=\tfrac{1}{5}m(a^2+b^2)\omega=\tfrac{4}{15}\pi\sigma\mathrm{a}^5(\sec\beta\sec\gamma)^{\frac{1}{3}}(1+\cos^2\beta)(4\pi\sigma)^{\frac{1}{2}}\left(\frac{\omega^2}{4\pi\sigma}\right)^{\frac{1}{2}}.$$

Thus
$$\mu=\tfrac{1}{5}\sqrt{3}(\sec\beta\sec\gamma)^{\frac{1}{3}}(1+\cos^2\beta)\left(\frac{\omega^2}{4\sigma}\right)^{\frac{1}{2}}. \quad . \quad . \quad . \quad (25)$$

The function (25) is the quantity which will be tabulated.

Again
$$(\tfrac{4}{3}\pi\sigma)^2\mathrm{a}^5\epsilon=\tfrac{1}{2}(\tfrac{4}{3}\pi\sigma)^{\frac{3}{2}}\mathrm{a}^7\mu\cdot\omega=\tfrac{1}{2}\sqrt{3}(\tfrac{4}{3}\pi\sigma)^2\mathrm{a}^5\cdot\mu\left(\frac{\omega^2}{4\pi\sigma}\right)^{\frac{1}{2}},$$

so that
$$\epsilon=\tfrac{1}{2}\sqrt{3}\mu\left(\frac{\omega^2}{4\pi\sigma}\right)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (26)$$

The function (26) is the quantity which will be tabulated.

Thus in the tables the unit of moment of momentum is taken as $(\frac{4}{3}\pi\sigma)^{\frac{1}{2}}a^5$, or $m^{\frac{1}{2}}a^{\frac{1}{2}}$, and the unit of energy as $(\frac{4}{3}\pi\sigma)^2a^5$ or m^2/a.

It remains to evaluate the intrinsic energy, or the energy required to expand the ellipsoid against its own gravitation, into a condition of infinite dispersion.

If dt be an element of volume, then this energy is

$$-\tfrac{1}{2}\iiint V\sigma dt$$

integrated throughout the ellipsoid.

This will be denoted by $(\frac{4}{3}\pi\sigma)^2a^5(i-1)$, or $m^2a^{-1}(i-1)$, so that i will be positive.

Now $V=Lx^2+My^2+Nz^2+P$, and if we denote by A, B, C, the principal moments of inertia of the ellipsoid, we have

$$\iiint x^2\sigma\, dt=\tfrac{1}{2}(B+C-A)=\tfrac{1}{5}ma^2,$$

and similarly, $\iiint y^2\sigma\, dt=\tfrac{1}{5}mb^2$, $\iiint z^2\sigma\, dt=\tfrac{1}{5}mc^2$.

Also $$\iiint \sigma dt=m.$$

Hence $$\frac{m^2}{a}(i-1)=\tfrac{1}{10}m[La^2+Mb^2+Nc^2+5P]$$

$$=\tfrac{1}{10}ma^2(\sec\beta\sec\gamma)^{\frac{1}{3}}[L+M\cos^2\beta+N\cos^2\gamma+5Pa^{-2}].$$

But if we take the values of L, M, N given in (15), and note that

$$P=\pi a^3\cos\beta\cos\gamma\cdot\frac{2}{a\sin\gamma}F,$$

it easily follows that

$$L+M\cos^2\beta+N\cos^2\gamma+Pa^{-2}=0.$$

Hence $$\frac{m^2}{a}(i-1)=-\tfrac{2}{5}ma^2(\sec\beta\sec\gamma)^{\frac{1}{3}}\cdot Pa^{-2}$$

$$=-\tfrac{2}{5}ma^2(\sec\beta\sec\gamma)^{\frac{1}{3}}\cdot\tfrac{3}{4}\frac{m}{a^2}\cdot\frac{2}{a\sin\gamma}F$$

$$=-\tfrac{3}{5}\frac{m^2}{a}\frac{(\cos\beta\cos\gamma)^{\frac{1}{3}}}{\sin\gamma}F.$$

Therefore $$i=1-\tfrac{3}{5}\frac{(\cos\beta\cos\gamma)^{\frac{1}{3}}}{\sin\gamma}F. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (27)$$

For a sphere γ becomes infinitely small, and F becomes equal to γ, so that $F/\sin\gamma=1$. Thus $i-1=-\frac{3}{5}$. Therefore the exhaustion of

energy of a sphere of radius a is $\frac{2}{5}m^2/a$; which is the known result. For an ellipsoid of revolution $\alpha=0$, and $\beta=0$, and $F=\gamma$; so that

$$i=1-\tfrac{3}{5}\gamma\frac{\cos^{\frac{1}{2}}\gamma}{\sin\gamma}.$$

The function (27) is the quantity tabulated below. It seemed preferable to tabulate a positive quantity, and it is on this account that the intrinsic energy corresponding to the infinitely long ellipsoid is entered as unity.

Having now obtained all the necessary formulæ, we may proceed to consider the solution of the problem.

We have to solve

$$\sec^2\alpha\sec^2\zeta\,E-(2F-E)-\tan\zeta\sec^2\alpha\sec^2\delta=0, \quad . \quad . \quad (28)$$

where $\tan\zeta=\sin\alpha\tan\beta\cos\gamma$, $\tan\delta=\sin\beta=\sin\alpha\sin\gamma$,

and $$F=\int_0^\gamma\frac{d}{\cos\beta}, \qquad E=\int_0^\gamma\cos\beta\,d\gamma.$$

The axes of the ellipsoid are

$$\frac{a}{a}=(\sec\beta\sec\gamma)^{\frac{1}{2}}, \qquad \frac{b}{a}=\frac{a}{a}\cos\beta, \qquad \frac{c}{a}=\frac{a}{a}\cos\gamma. \quad . \quad (29)$$

If e_1, e_2, e_3 are the eccentricities of the sections through ca, cb, ab respectively, we have

$$e_1=\sin\beta, \qquad e_2=\sin\gamma, \qquad e_3=\cos\alpha\sin\gamma\sec\beta. \quad . \quad (30)$$

Having obtained the solution, we have to compute

$$\frac{\omega^2}{4\pi\sigma}=\cot\beta\,\mathrm{cosec}\,\beta\cot\gamma(F-E)+\cot^3\beta\cos\beta\sec^2\alpha E-\cos^2\beta\cot^2\gamma\sec^2\alpha^*$$
$$. \quad . \quad . \quad (31)$$

Then we next compute μ and ϵ and i from the formulæ (25), (26), (27).

The functions F and E are tabulated in Table IX of the second volume of Legendre's 'Traité des Fonctions Elliptiques,' in a table of double entry for α and γ for each degree.

The solution of (28) by trial and error was laborious, as it was necessary to work with all the accuracy attainable with logarithms of seven figures.

The method adopted was to choose an arbitrary value of γ, and

* As stated above, some of the computations were actually made from the formula (20).

Solutions of Jacobi's Problems.

Auxiliary angles.			Axes.			Eccentricities of sections.			Ang. vel.	Mom. of momentum.	Energy.		
γ.	α.	β.	a/a.	b/a.	c/a.	$(ac.)$ e_1.	$(bc.)$ e_2.	$(ab.)$ e_3.	$\omega^2/4\pi\sigma$.	μ.	Kinetic. ϵ.	Intrinsic. i.	Total. E.
54° 21′ 27″	0° 0′	0° 0′	1·1972	1·1972	·6977	·81267	·81267	·0000	·09356	·30375	·08046	·41495	·49541
55	17¾	14¼	1·216	1·179	·698	·819	·806	·246	·094	·304	—	—	—
57	34¾	28½	1·279	1·123	·696	·839	·784	·478	·093	·306	—	—	—
60	49 7	40 54	1·3831	1·0454	·6916	·8660	·7500	·6547	·09060	·3134	·0817	·4188	·5005
65	64 19	54 46	1·6007	·9235	·6765	·9063	·6807	·8168	·08295	·3407	·0850	·4394	·5244
70	74 12	64 43	1·899	·8111	·6494	·9397	·5991	·9042	·07047	·3920	·0901	·4189	·5390
75	81 4	72 36	2·346	·7019	·6072	·9659	·5014	·9542	·0536	·4809	·0964	·4808	·5772
80	86	79¼	3·136	·5858	·5445	·9848	·3684	·9824	·0334	·644	·1019	·5334	·6353
85	88⅝	84⅝	5·04	·45	·44	·996	·228	·996	·013	1·016	·101	·645	·746
90	90	90	∞	·00	·00	1·000	·000	1·000	·000	∞	·000	1·000	1·000

N.B.—*The moment of momentum of the system is* $(\frac{4}{3}\pi\sigma)^{\frac{3}{5}}a^5.\mu$, *or* $m^{\frac{3}{5}}a^{\frac{1}{2}}\mu$; *the kinetic energy is* $(\frac{4}{3}\pi\sigma)^2a^5.\epsilon$, *or* $m^2a^{-1}\epsilon$, *and the intrinsic energy is* $(\frac{4}{3}\pi\sigma)^2a^5.(i-1)$, *or* $m^2a^{-1}(i-1)$, *but in the above table unity has been added to make the results positive;* $E=\epsilon+i$.

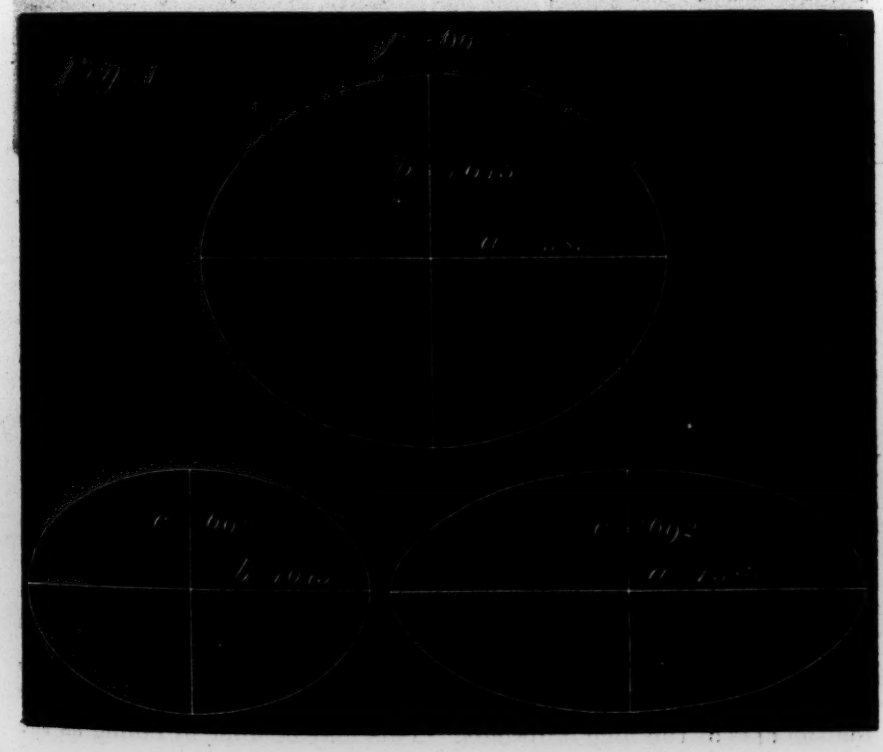
then by trial and error to find two values of α one degree apart, one of which made the left-hand side of (28) positive, and the other negative.

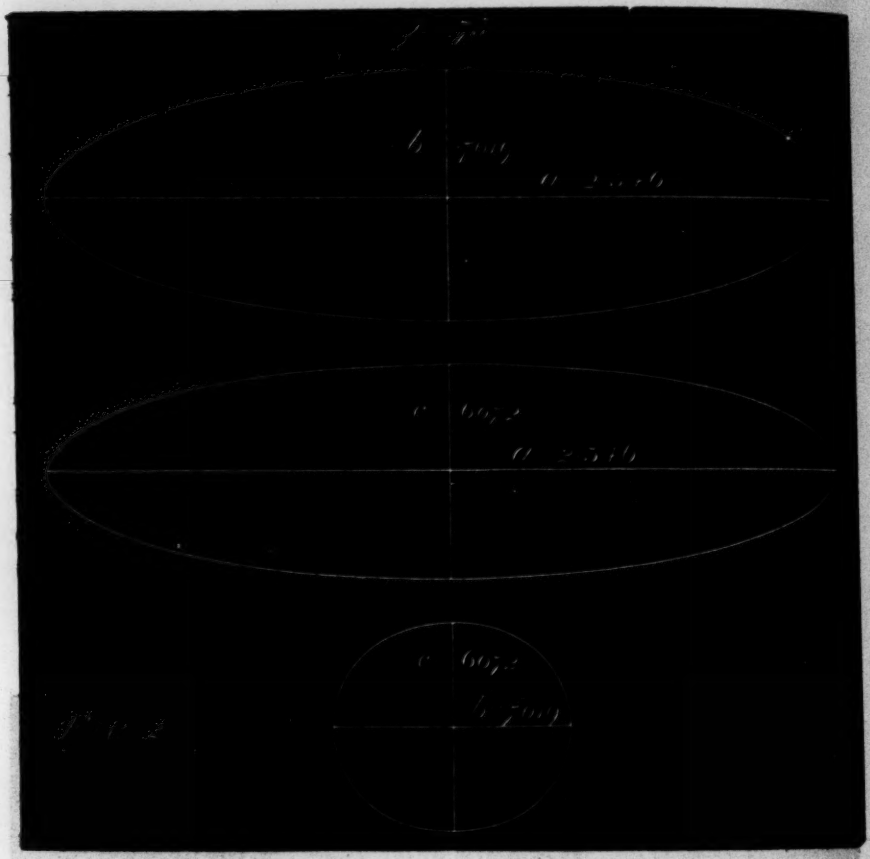
The smallest value of γ is 54° 21′ 27″, but after that value integral degrees for γ were always chosen.

The solutions for $\gamma=55°$ and 57° could not be found very exactly from the elliptic integrals with logarithms of only seven figures, but the solutions were confirmed by the approximate formulæ (21) and (22). The solution for $\gamma=80°$ was confirmed by the approximate formulæ (23) and (24), and that for $\gamma=85°$ was only computed therefrom, since when $\gamma=80°$ the approximate formula gave nearly identical results with the exact one.

The solution obtained is embodied in the table opposite. The first three columns give the auxiliary angles γ, α, β, from which the remaining results are computed.

As a graphical result is much more intelligible than a numerical one, I have given two figures, showing the three principal sections in two cases, namely, where $\gamma=60°$, and $\gamma=75°$. For these figures a is taken as 2 cm., so that the volume of fluid is $\frac{4}{3}\pi\times 2^3$ cubic cm.





It will be noticed that the longer the ellipsoid the slower it rotates. It is interesting to observe that while the angular velocity continually diminishes, the moment of momentum continually increases. The long ellipsoids are very nearly ellipsoids of revolution about an axis perpendicular to that of rotation. Thus in fig. 2 the section through b and c is not much flattened.

The most remarkable point is that there is a maximum of kinetic energy when a/a is about 3, or when the length of the ellipsoid is about five times its diameter. However, notwithstanding this maximum of kinetic energy, the total energy always increases with the length of the ellipsoid.

The kinetic energy is the product of two factors, one of which always increases, and the other of which always diminishes; thus it is obvious that it must have a maximum. The result was, however, quite unforeseen, and it seems worth while to obtain simpler formulæ for the case of the long ellipsoids. This may be done by taking as the parameter a/a, or the length of the ellipsoid, instead of γ.

From the table we see that in the later entries β is very nearly

equal to γ, and that α becomes very nearly equal to 90°. Hence we may put $\alpha=90°$, and $\beta=\gamma$.

Thus, approximately,

$$\frac{a}{\mathrm{a}} = (\sec\beta\sec\gamma)^{\frac{1}{3}} = (\sec\gamma)^{\frac{2}{3}}$$

and $$\cos\gamma=\left(\frac{\mathrm{a}}{a}\right)^{\frac{3}{2}}, \qquad \gamma=\tfrac{1}{2}\pi-\left(\frac{\mathrm{a}}{a}\right)^{\frac{3}{2}}.$$

The axes of the ellipsoid are

$$a,\ \left(\frac{\mathrm{a}^3}{a}\right)^{\frac{1}{2}},\ \left(\frac{\mathrm{a}^3}{a}\right).$$

Now if in formula (24) we only retain the higher powers of $\tan\gamma$, we have

$$\frac{\omega^2}{4\pi\sigma}=\frac{\sin\gamma\cos\gamma}{\tan\gamma}\left[\frac{1}{\sin\gamma}\log_e\cot\left(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma\right)-\tfrac{3}{2}\right]$$

$$=\tfrac{3}{2}\frac{\cos^2\gamma}{\sin\gamma}\left[\tfrac{2}{3}\log_e\cot\left(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma\right)-\sin\gamma\right].$$

But

$$\log_e\cot\left(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma\right)=\log_e\frac{1+\sin\gamma}{\cos\gamma}=\log_e 2+\tfrac{3}{2}\log_e\frac{a}{\mathrm{a}}.$$

Therefore writing $1-\frac{2}{3}\log_e 2=\mathrm{C}$, so that $\mathrm{C}=0{\cdot}3573$, we have

$$\frac{\omega^2}{4\pi\sigma}=\tfrac{3}{2}\left(\frac{\mathrm{a}}{a}\right)^3\left[\log_e\frac{a}{\mathrm{a}}-\mathrm{C}\right].$$

If we put $a/\mathrm{a}=5{\cdot}042$, this formula gives $\omega^2/4\pi\sigma=0{\cdot}01264$. The full value in the preceding tables was 0·0131; thus even with so short an ellipsoid as this, the results agree within 4 per cent. With rougher approximation we have

$$\frac{\omega^2}{4\pi\sigma}=\tfrac{3}{2}\left(\frac{\mathrm{a}}{a}\right)^3\log_e\frac{a}{\mathrm{a}},$$

of which the limit, when a is large, is zero.

For the moment of momentum we have

$$\mu=\frac{3^{\frac{1}{2}}}{5}(\sec\beta\sec\gamma)^{\frac{1}{3}}(1+\cos^2\beta)\left(\frac{\omega^2}{4\pi\sigma}\right)^{\frac{1}{2}}$$

$$=\frac{3}{5.2^{\frac{1}{3}}}\left(\frac{a}{\mathrm{a}}\right)^{\frac{1}{2}}\left(1+\frac{\mathrm{a}^3}{a^3}\right)\left(\log_e\frac{a}{\mathrm{a}}-\mathrm{C}\right)^{\frac{1}{2}},$$

or, with rougher approximation,

$$\mu = \frac{3}{5.2^{\frac{1}{3}}}\left(\frac{a}{\mathrm{a}}\right)^{\frac{1}{2}}\left(\log_e\frac{a}{\mathrm{a}}\right)^{\frac{1}{2}},$$

of which the limit is infinite.

Again,
$$\epsilon = \frac{3^{\frac{1}{3}}}{2}\mu\left(\frac{\omega^2}{4\pi\sigma}\right)$$
$$= \tfrac{9}{20}\frac{\mathrm{a}}{a}\left[1+\frac{\mathrm{a}^3}{a^3}\right]\left[\log_e\frac{a}{\mathrm{a}}-\mathrm{C}\right].$$

Now the function $\frac{\mathrm{a}}{a}\left(\log_e\frac{a}{\mathrm{a}}-\mathrm{C}\right)$ has a maximum, when $\log_e\frac{a}{\mathrm{a}}=1+\mathrm{C}$ $=1{\cdot}3573$, that is when $\frac{\mathrm{a}}{a}=1{\cdot}696$.

On comparison with our tables it is obvious that the approximation is bad, and that the true solution for a maximum is considerably different from the above. Nevertheless this investigation shows that there is actually a maximum of kinetic energy.

Since
$$\mathrm{F}=\log_e\cot\left(\tfrac{1}{4}\pi-\tfrac{1}{2}\gamma\right)=\tfrac{3}{2}\left[\log_e\frac{a}{\mathrm{a}}+\tfrac{2}{3}\log_e 2\right],$$

we have

$$i = 1-\frac{\tfrac{3}{5}(\cos\beta\cos\gamma)^{\frac{1}{3}}}{\sin\gamma}\mathrm{F} = 1-\tfrac{9}{10}\left(\frac{\mathrm{a}}{a}\right)^3\left[\log_e\frac{a}{\mathrm{a}}+\tfrac{2}{3}\log_e 2\right].$$

If we like we may express these several results in terms of the minor and major axes of the ellipsoid, for $b=c=\frac{\mathrm{a}^{\frac{3}{2}}}{a^{\frac{1}{2}}}$, and therefore $\mathrm{a}^3=c^2a$.

Thus
$$\left.\begin{aligned}
\frac{\omega^2}{4\pi\sigma} &= \frac{c^2}{a^2}\left[\log_e\frac{a}{c}-\tfrac{3}{2}\mathrm{C}\right].\\
\mu &= \frac{3^{\frac{1}{3}}}{5}\left(\frac{a}{c}\right)^{\frac{1}{3}}\left[1+\frac{c^2}{a^2}\right]\left[\log_e\frac{a}{c}-\tfrac{3}{2}\mathrm{C}\right]^{\frac{1}{2}}.\\
\epsilon &= \tfrac{3}{10}\left(\frac{c}{a}\right)^{\frac{2}{3}}\left(1+\frac{c^2}{a^2}\right)\left(\log_e\frac{a}{c}-\tfrac{3}{2}\mathrm{C}\right).\\
i &= 1-\tfrac{3}{5}\frac{a^2}{c^2}\log_e\frac{2a}{c}.
\end{aligned}\right\}$$

IV. "On the Dynamical Theory of the Tides of Long Period." By G. H. Darwin, LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge.

Received November 5, 1886.

In the following note an objection is raised against Laplace's method of treating these tides, and a dynamical solution of the problem, founded on a paper by Sir William Thomson, is offered.

Let θ, ϕ be the colatitude and longitude of a point in the ocean, let ξ and $\eta \sin\theta$ be the displacements from its mean position of the water occupying that point at the time t, let $\mathfrak{h}$ be the height of the tide, and let $\mathfrak{e}$ be the height of the tide according to the equilibrium theory; let n be the angular velocity of the earth's rotation, g gravity, a the earth's radius, and γ the depth of the ocean at the point θ, ϕ.

Then Laplace's equations of motion for tidal oscillations are—

$$\left.\begin{aligned} \frac{d^2\xi}{dt^2} - 2n\sin\theta\cos\theta\frac{d\eta}{dt} &= -\frac{g}{a}\frac{d}{d\theta}(\mathfrak{h}-\mathfrak{e}) \\ \sin\theta\frac{d^2\eta}{dt^2} + 2n\cos\theta\frac{d\xi}{dt} &= -\frac{g}{a\sin\theta}\frac{d}{d\phi}(\mathfrak{h}-\mathfrak{e}) \end{aligned}\right\} \quad . \quad . \quad . \quad (1)$$

And the equation of continuity is—

$$\mathfrak{h}a + \frac{1}{\sin\theta}\frac{d}{d\theta}(\gamma\xi\sin\theta) + \gamma\frac{d\eta}{d\phi} = 0. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The only case which will be considered here is where the depth of the ocean is constant, and we shall only treat the oscillations of long period in which the displacements are not functions of the longitude.

As the motion to be considered only involves steady oscillation, we assume—

$$\left.\begin{aligned} \mathfrak{e} &= e\cos(2nft+\alpha) \\ \mathfrak{h} &= h\cos(2nft+\alpha) \\ \xi &= x\cos(2nft+\alpha) \\ \eta &= y\sin(2nft+\alpha) \\ u &= h-e \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Hence, by substitution in (1), we have

$$\left.\begin{aligned} xf^2 + yf\sin\theta\cos\theta &= \frac{1}{4m}\frac{du}{d\theta} \\ yf^2\sin\theta + xf\cos\theta &= 0 \end{aligned}\right\},$$

where $$m=\frac{n^2a}{g}.$$

Whence $$x(f^2-\cos^2\theta)=\frac{1}{4m}\frac{du}{d\theta},$$

$$y\sin\theta(f^2-\cos^2\theta)=-\frac{1}{4m}\frac{\cos\theta}{f}\frac{du}{d\theta}.$$

Then substituting for x and y in (2), which, when γ is constant and η is not a function of ϕ, becomes

$$ha+\frac{\gamma}{\sin\theta}\frac{d}{d\theta}(\xi\sin\theta)=0,$$

we get $$\frac{\gamma}{\sin\theta}\frac{d}{d\theta}\left[\frac{\sin\theta\, du/d\theta}{f^2-\cos^2\theta}\right]+4ma(u+e)=0.$$

This is Laplace's equation for tidal oscillations of the first kind.* In these tides f is a small fraction, being about $\frac{1}{28}$ in the case of the fortnightly tide, and e the coefficient in the equilibrium tide is equal to $E(\frac{1}{3}-\cos^2\theta)$, where E is a known function of the elements of the orbit of the tide-generating body, and of the obliquity of the ecliptic.

If now we write $\beta=4ma/\gamma$, and $\mu=\cos\theta$, our equation becomes

$$\frac{d}{d\mu}\left[\frac{1-\mu^2}{\mu^2-f^2}\frac{du}{d\mu}\right]=\beta[u+E(\tfrac{1}{3}-\mu^2)]. \quad . \quad . \quad . \quad . \quad (4)$$

In treating these oscillations Laplace does not use this equation, but seeks to show that friction suffices to make the ocean assume at each instant its form of equilibrium. His conclusion is no doubt true, but the question remains as to what amount of friction is to be regarded as sufficing to produce the result, and whether oceanic tidal friction can be great enough to have the effect which he supposes it to have.

The friction here contemplated is such that the integral effect is represented by a retarding force proportional to the velocity of the fluid relatively to the bottom. Although proportionality to the square of the velocity would probably be nearer to the truth, yet Laplace's hypothesis suffices for the present discussion. In oscillations of the class under consideration, the water moves for half a period north, and then for half a period south.

Now in systems where the resistances are proportional to velocity, it is usual to specify the resistance by a modulus of decay, namely, that period in which a velocity is reduced by friction to e^{-1} or $1\div2{\cdot}783$ of its initial value; and the friction contemplated by Laplace

* 'Mécanique Céleste.'

is such that the modulus of decay is short compared with the semi-period of oscillation.

The quickest of the tides of long period is the fortnightly tide, hence for the applicability of Laplace's conclusion, the modulus of decay must be short compared with a week. Now it seems practically certain that the friction of the ocean bed would not much affect the velocity of a slow ocean current in a day or two. Hence we cannot accept Laplace's hypothesis as to the effect of friction.

We now, therefore, proceed to the solution of the equation of motion when friction is entirely neglected.

The solution here offered is indicated in a footnote to a paper by Sir William Thomson ('Phil. Mag.,' vol. 50, 1875, p. 280), but has never been worked out before.

The symmetry of the motion demands that u, when expanded in a series of powers of μ, shall only contain even powers of μ.

Let us assume then

$$\frac{1}{\mu^2-f^2}\frac{du}{d\mu}=B_1\mu+B_3\mu^3+\ \ldots\ +B_{2i+1}\mu^{2i+1}+\ \ldots$$

Then

$$\frac{1-\mu^2}{\mu^2-f^2}\frac{du}{d\mu}=B_1\mu+(B_3-B_1)\mu^3+\ \ldots\ +(B_{2i+1}-B_{2i-1})\mu^{2i+1}+\ \ldots$$

$$\frac{d}{d\mu}\left[\frac{1-\mu^2}{\mu^2-f^2}\frac{du}{d\mu}\right]=B_1+3(B_3-B_1)\mu^2+\ \ldots\ +(2i+1)(B_{2i+1}-B_{2i-1})\mu^{2i}+$$

. . . . (5)

Again

$$\frac{du}{d\mu}=-f^2B_1\mu+(B_1-f^2B_3)\mu^3+\ \ldots\ +(B_{2i-1}-f^2B_{2i+1})\mu^{2i+1}+\ \ldots$$

$$u=C-\tfrac{1}{2}f^2B_1{}^2+\tfrac{1}{4}(B_1-f^2B_3)\mu^4+\ \ldots\ +\tfrac{1}{2i}(B_{2i-3}-f^2B_{2i-1})\mu^{2i}+\ \ldots,$$

. . . (6)

where C is a constant.

Then substituting from (5) and (6) in (4), and equating to zero the successive coefficients of the powers of μ, we find,

$$\left.\begin{aligned}
&C=-\tfrac{1}{3}E+B_1/\beta\\
&B_3-B_1(1-\tfrac{1}{2.3}f^2\beta)+\tfrac{1}{3}\beta E=0\\
&\quad\ldots\ldots\ldots\ldots\\
&B_{2i+1}-B_{2i-1}(1-\tfrac{1}{2i(2i+1)}f^2\beta)-\tfrac{1}{2i(2i+1)}\beta B_{2i-3}=0\\
&\quad\ldots\ldots\ldots\ldots
\end{aligned}\right\}\ \ldots\ (7)$$

Thus the constants C and B_3, B_5, &c., are all expressible in terms of B_1.

We may remark that if

$$-\tfrac{1}{2}\cdot\tfrac{1}{3}\cdot\beta B_{-1}=\tfrac{1}{3}\beta E, \text{ or } B_{-1}=-2E,$$

then the general equation of condition in (7) may be held to apply for all values of i from 1 to infinity.

Let us now write it in the form—

$$\frac{B_{2i+1}}{B_{2i-1}}=1-\frac{1}{2i(2i+1)}f^2\beta+\frac{1}{2i(2i+1)}\beta\frac{B_{2i-3}}{B_{2i-1}}. \quad . \quad . \quad . \quad . \quad (8)$$

When i is large, B_{2i+1}/B_{2i-1} either tends to become infinitely small, or it does not do so.

Let us suppose that it does not tend to become infinitely small. Then it is obvious that the successive B's tend to become equal to one another, and so also do the coefficients $B_{2i-1}-f^2B_{2i+1}$ in the expression for $du/d\mu$.

Hence $\dfrac{du}{d\mu}=L+\dfrac{M}{1-\mu^2}$, where L, M are finite, for all values of μ.

Hence $\dfrac{du}{d\theta}=-L\sqrt{1-\mu^2}+\dfrac{M}{\sqrt{1-\mu^2}}$, and therefore x is infinite when $\mu=1$ at the pole, and $d\xi/dt$ is infinite there also.

Hence the hypothesis, that B_{2i+1}/B_{2i-1} does not tend to become infinitely small, gives us infinite velocity at the pole. But with a globe covered with water this is impossible, the hypothesis is negatived, and B_{2i+1}/B_{2i-1} tends to become infinitely small.

This being established let us write (8) in the form—

$$\frac{B_{2i-1}}{B_{2i-3}}=\frac{-\frac{1}{2i(2i+1)}\beta}{1-\frac{1}{2i(2i+1)}f^2\beta-B_{2i+1}/B_{2i-1}}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

By repeated applications of (9) we have in the form of a continued fraction

$$\frac{B_{2i-1}}{B_{2i-3}}=\cfrac{-\frac{1}{2i(2i+1)}\beta}{1-\frac{1}{2i(2i+1)}f^2\beta}+\cfrac{\frac{1}{(2i+2)(2i+3)}\beta}{1-\frac{1}{(2i+2)(2i+3)}f^2\beta}+\cfrac{\frac{1}{(2i+4)(2i+5)}\beta}{1-\frac{1}{(2i+4)(2i+5)}f^2\beta}+\&c.$$

$$. \quad . \quad . \quad (10)$$

And we know that this is a continuous approximation, which must hold in order to satisfy the condition that the water covers the whole globe.

Let us denote this continued fraction by $-N_i$.

Then, if we remember that $B_{-1}=-2E$, we have

$$B_1=2EN_1, \qquad \frac{B_3}{B_1}=-N_2, \qquad \frac{B_5}{B_3}=+N_3, \text{ \&c.,}$$

so that

$$B_3=-2EN_1N_2, \qquad B_5=-2EN_1N_2N_3, \qquad B_7=-2EN_1N_2N_3N_4, \text{ \&c.}$$

and

$$C=-\tfrac{1}{3}E+2\frac{EN_1}{\beta}.$$

Then the height of tide h is equal to $h\cos(2nft+\alpha)$, the equilibrium tide ζ is equal to $E(\tfrac{1}{3}-\mu^2)\cos(2nft+\alpha)$, and we have

$$h=u+E(\tfrac{1}{3}-\mu^2)$$

$$=C+\tfrac{1}{3}E-(E+\tfrac{1}{2}f^2B_1)\mu^2+\tfrac{1}{4}(B_1-f^2B_3)\mu^4+\tfrac{1}{6}(B_3-f^2B_5)\mu_6+\ldots$$

$$\frac{h}{E}=\frac{2N_1}{\beta}-(1+f^2N_1)\mu^2+\tfrac{1}{2}N_1(1+f^2N_2)\mu^4-\tfrac{1}{3}N_1N_2(1+f^2N_3)\mu_6+\ldots$$

Now when $\beta=40$, we have $\gamma=\frac{1}{40}\times 4ma=\frac{1}{2800}a=7260$ feet; so that $\beta=40$ gives an ocean of 1200 fathoms.

With this value of β, and with $f=\cdot0365012$, which is the value for the fortnightly tide, I find

$N_1=3\cdot040692$, $N_2=1\cdot20137$, $N_3=\cdot66744$, $N_4=\cdot42819$, $N_5=\cdot29819$,

$N_6=\cdot21950$, $N_7=\cdot16814$, $N_8=\cdot13287$, $N_9=\cdot107$, $N_{10}=\cdot1$, &c.

These values give

$$\frac{2}{\beta}N_1=\cdot15203, \qquad 1+f^2N_1=1\cdot0041, \qquad \tfrac{1}{2}N_1(1+f^2N_2)=1\cdot5228,$$

$$\tfrac{1}{3}N_1N_2(1+f^2N_3)=1\cdot2187, \qquad \tfrac{1}{4}N_1N_2N_3(1+f^2N_4)=\cdot6099,$$

$$\tfrac{1}{5}N_1\ldots N_4(1+f^2N_5)=\cdot2089 \qquad \tfrac{1}{6}N_1\ldots N_5(1+f^2N_6)=\cdot0519,$$

$$\tfrac{1}{7}N_1\ldots N_6(1+f^2N_7)=\cdot0098, \qquad \tfrac{1}{8}N_1\ldots N_7(1+f^2N_8)=\cdot0014,$$

$$\tfrac{1}{9}N_1\ldots N_8(1+f^2N_9)=\cdot00017, \text{ \&c.}$$

So that

$$\frac{h}{E}=\cdot1520-1\cdot0041\mu^2+1\cdot5228\mu^4-1\cdot2187\mu^6+\cdot6099\mu^8-\cdot2089\mu^{10}$$
$$+\cdot0519\mu^{12}-\cdot0098\mu^{14}+\cdot0014\mu^{16}-\cdot0002\mu^{18}+\ldots$$

At the pole, where $\mu=1$, the equilibrium tide is $-\frac{2}{3}E$; at the equator it is $+\frac{1}{3}E$.

Now at the pole $h=-E\times\cdot1037=-\frac{2}{3}E\times\cdot1556$,

and at the equator $h=+E\times\cdot1520=\ \frac{1}{3}E\times\cdot4561$.

In a second case, namely, with an ocean four times as deep, so that $\beta=10$, I find

$$\frac{h}{E} = \cdot 2363 - 1\cdot 0016\mu^2 + \cdot 5910\mu^4 - \cdot 1627\mu^6 + \cdot 0258\mu^8 - \cdot 0026\mu^{10} + \cdot 0002\mu^{12} - \ldots$$

At the pole $h = -E \times \cdot 3137 = -\frac{2}{3}E \times \cdot 471$,

at the equator $h = +E \times \cdot 2363 = +\frac{1}{3}E \times \cdot 709$.

With a deeper ocean we should soon arrive at the equilibrium value for the tide, for N_2, N_3, &c., become very small, and $2N_1/\beta$ becomes equal to $\frac{1}{3}$.

These two cases, $\beta = 40$, $\beta = 10$, are two of those for which Laplace has given solutions in the case of the semi-diurnal and diurnal tides. We notice that, with such oceans as we have to deal with, the tide of long period is certainly less than half its equilibrium amount.

In Thomson and Tait's 'Natural Philosophy' (edition of 1883) I have made a comparison of the observed tides of long period with the equilibrium theory. The probable errors of the results are large, but not such as to render them worthless, and in view of the present investigation it is surprising to find that on the average the tides of long period amount to as much as two-thirds of their equilibrium value.

The investigation in the 'Natural Philosophy' was undertaken in the belief of the correctness of Laplace's view as to the tides of long period, and was intended to evaluate the effective rigidity of the earth's mass.

The present result shows us that it is not possible to attain any estimate of the earth's rigidity in this way, but as the tides of long period are distinctly sensible, we may accept the investigation in the 'Natural Philosophy' as generally confirmatory of Thomson's view as to the great effective rigidity of the whole earth's mass.

There is one tide, however, of long period of which Laplace's argument from friction must hold true. In consequence of the regression of the nodes of the moon's orbit there is a minute tide with a period of nearly nineteen years, and in this case friction must be far more important than inertia. Unfortunately this tide is very minute, and as I have shown in a Report for 1886 to the British Association on the tides, it is entirely masked by oscillations of sea level produced by meteorological or other causes.

Thus it does not seem likely that it will ever be possible to evaluate the effective rigidity of the earth's mass by means of tidal observations.

Presents, November 18, 1886.

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"On the Method of Condensation in Calorimetry." By J. JOLY, B.E., Assistant to the Professor of Civil Engineering, Trinity College, Dublin. Communicated by Professor FITZGERALD, F.R.S. Received June 28. Read November 18, 1886.

[PLATES 6 and 7.]

Introduction.

This paper is devoted generally to experimental verification of a method of condensation applied to the determination of specific heat. The extension of the method to the calorimetry of latent heat of gasification has been the subject of some experiments, but the further delay necessary to complete these compels me to defer their consideration. Similarly its application, theoretically practicable, to the direct determination of the specific heats of gases at constant volume, is not here considered.

In another paper (p. 250) some results bearing on the possible value of the method in physical mineralogy and in determinative mineralogy are given. The method was originally devised in the hope that it might be of service in this branch of investigation.

On the subject of this paper a couple of brief notices only have up to the present been published. One of these arose out of an informal notice brought before the Royal Dublin Society;* the second from a verbal account laid before the Dublin University Experimental Science Association.†

To the facilities extended to me by Prof. Fitzgerald, in the Physical Laboratory of Trinity College, the whole of the experimental verification of the method is due. I have not attempted to acknowledge anywhere adequately how much is due to his advice and suggestions.

Theory of the Method.

If W grams of a substance at the temperature $t_1°$ of its surroundings be immersed in an atmosphere of saturated vapour, steam suppose, the temperature of which at the prevailing atmospheric pressure is $t_2°$, the substance experiences a rise in temperature of $t_2°-t_1°$, and absorbs a number of calories, $WS(t_2°-t_1°)$, where S is the specific heat of the body. Also if λ is the latent heat of steam at the temperature $t_2°$, a quantity of steam weighing w grams is condensed, so that

$$WS(t_2°-t_1°) = w\lambda.$$

If in this equation λ be known, the value of S may be found by

* 'Irish Times,' February 17, 1885.

† 'Dublin University Review,' June, 1885.

observation of the remaining quantities. The value of λ has been accurately determined by Regnault and recorded for all values of t_2° likely to occur. By similar observations, knowing S, λ may be the unknown quantity sought for by experiment, and determined from the equation. A substance of known specific heat being in this case immersed in a vapour of unknown latent heat.

It is evident that of the quantities to be measured the measurement of the value of w alone presents any difficulty. I have found, however, that w is accurately determined by weighing the quantity of vapour condensing on the surface of the substance in its transition of temperature from t_1° to t_2°; and the calorimeters described in this paper are in short contrivances permitting the accurate determination of the weight of condensation as it gathers and falls from the surface of a substance suddenly surrounded by an atmosphere of steam. As the process is one principally of weighing, I have called the apparatus a *gravimetric calorimeter*.

Up to the present I have only employed the condensation of water-vapour in effecting determinations of specific heat gravimetrically. There are evidently sufficient reasons in favour of its use, chief among these is the accurate knowledge we possess of its latent heat through Regnault's experiments. But the ease with which it may be generated, and its chemical inertness when in presence of the materials of the calorimeter, are also important. The range of temperature too involved is sufficiently considerable, and, for my purpose, allowed of convenient comparison with the data of Regnault and others on specific heat obtained generally through this same range. By the use of other vapours greater weights of condensation are obtainable, but this is only of consequence when dealing with very small quantities of matter. The variations, however, in the upper limit of temperature thus obtainable might be otherwise important.

Description of a Gravimetric Calorimeter.

Whatever vapour be used in this method of calorimetry, two chief conditions must be attended to in its employment: (1) sudden replacement of the air surrounding the substance by the vapour to be condensed; (2) perfect mechanical conditions permitting the evaluation of the weight condensed while the substance is still surrounded with the vapour.

To effect the first of these conditions we might evidently either introduce the substance quickly into a chamber filled with and supplied with the vapour, or, maintaining the substance throughout in one chamber, replace the air around it by sudden and rapid inflow of the vapour. In choosing the last we dispense both with much contrivance and manipulation involved in the first. The error of trans-

ference obtaining in all other methods is almost entirely avoided, and in fact the facilities offered by the method are more fully availed of. We have in fact a change of state method which involves, as I hope to show, no sensible radiation errors, the simplest thermometry and apparatus, and no manipulative difficulties whatsoever.

Plate 6 is from a photograph of the apparatus. The calorimeter consists of a nearly spherical chamber of very thin spun brass, 10·5 cm. in diameter, made up of two hemispheres meeting on thin ground flanges. These hemispheres are carried by standards which slide upon the centre one of three rails forming a light girder. This girder is supported on three feet, and is arranged to stand in one assigned position inside the case of a large physical balance. I used it for a considerable time exactly in the position of the bridge used for supporting a beaker in taking specific gravities; that is, spanning one of the pans and passing between its stirrups. I subsequently found it necessary to remove the pan, substituting a counterpoise. It will be understood now that when the standards carrying the hemispheres are slid along the centre rail, the sphere can be completed inside the balance case, and when meeting about the centre of the rail, may be made to enclose a substance depending from the hook of the balance. To permit of the substance being weighed while thus enclosed in the sphere, a notch is provided in the flanges at their top meeting point. This notch is, for reasons to be explained later, cut in two little plugs of plaster of Paris, contained in cells provided for them. The sphere has two openings with short tubulures, one on each hemisphere, horizontal, and opposite one another. The larger of the two, 2·2 cm. diameter, opening to the front of the balance, serves for the admission of steam; the other, which when in use is prolonged by the addition of a couple of centimetres of rubber tubing, is about 1 cm. in diameter, and serves for the exit of steam at the rear of the balance.

To secure the more sudden admission of steam, a movable admission pipe fitting the front nozzle on a ground joint is provided. It is carried on a separate standard, also fitting the centre rail, and is connected with a small tin or copper boiler* by a flexible tube, so that when it and the rubber tube connexion have been first thoroughly heated by the passage of steam, it can be run up on the centre rail to meet the nozzle and discharge the steam directly into the sphere. Over the entire length of the calorimeter extends a rectangular tunnel-shaped covering of mahogany, made in halves, meeting at the centre of the girder, and provided with notches at their meeting faces, also cut in plaster of Paris, to permit the passage of the wire sup-

* I use a cylindrical copper boiler, 15 cm. diameter by 20 cm. high. This, heated by a strong Bunsen burner, evolves steam at the rate, I find, of about 0·4 gram per second. As it is important to introduce the steam as quickly as possible, a large boiler is advantageous.

porting the substance contained within the sphere. But one-half this covering is shown in the plate. It is borne upon the two outside rails of the girder, which between the rails is filled in with mahogany. The tunnel serves to isolate the calorimeter completely from the mechanism of the balance, and thus arranged I can detect no errors due to escape of heat. The apparatus might, however, also be arranged to stand beneath the balance, the balance being supported on a shelf, and a wire carried down from it to the calorimeter.

The little screws seen at the feet of the standards bear against the upper surface of the rail, enabling the standards to be adjusted for the accurate meeting of the ground joints. This is effected once for all. For smooth working the standards are clipped sideways against the rail by spring straps. Thumbscrews on top of the standards permit axial motion of the several parts, enabling the hemispheres to be adjusted for the free passage of the wire. The steamways are insulated from the standards by split rings of hard wood. Beneath the sphere, and accommodated by a gap in the central rail, a little vessel is placed for catching the water which drips from the sphere during experiment. The correct position of the girder is secured by means of a plate fixed to the floor of the balance, and provided with recesses to receive the feet of the girder. The girder is thus placed in position, and may be lifted in and out as required, without necessity of any readjustment.

The support for carrying the substance being experimented on, and the bucket for catching the condensed vapour dropping from it, are shown about full size in Plate 6. It is best to have these made of platinum. The conical bucket shown to one side of the carrier, and which, when in use, rests on the little claw beneath the latter, is of thin platinum foil. The rest is of platinum wire. This design secures slightness and strength—over 100 grams may be placed on the cross wires—and supports the substance so that it is freely exposed to the steam on all sides, while the bucket beneath can be conveniently removed for cleaning and drying. Buckets made of silver foil quickly tarnish, owing to impurities in the steam. They prove less constant in calorific capacity than those of platinum.*

Method of Making an Experiment.

In exemplification of the manipulation involved in making an experiment, I describe one made on cast zinc. The piece of zinc is probably nearly pure. It weighs 48·300 grams.

About an hour before we make the experiment it is placed on the carrier, which depends by a fine platinum wire from the hook of the

* All the apparatus described in this paper is the workmanship of Yeates, of Dublin, and leaves nothing to be desired.

balance. The hemispheres of the calorimeter are then run up on the rail from each side of the balance and closed around the carrier. They are placed so that the wire passes freely through the aperture provided. A thermometer graduated to the tenth part of a degree Centigrade is next inserted in the tubulure opening to the front of the balance, so that its bulb projects well into the sphere. The tunnel-shaped covering is placed in position, and the piece of zinc and the carrier counterpoised. The balance is sensitive to half milligrams.

We assume that the thermometer indicates closely the temperature of the zinc after the lapse of about an hour. At the end of that time the boiler is heated up, and steam passed briskly through the leading tube to heat it throughout. This tube is of a length permitting the boiler to be placed at a safe distance from the calorimeter. The thermometer, where it projects from the sphere, is now read, in doing which it is necessary to remove the front half of the tunnel for a moment. The temperature indicated is 10·73° (corrected at Kew), the last number by estimation.

The thermometer is now withdrawn and the admission pipe slid quickly along the rail up to the calorimeter, the leading tube being pinched for a moment during this brief operation. Steam is now flowing rapidly into the calorimeter; in a few seconds it begins to escape at the exit tube to the far side of the balance. When it is seen to come out freely a valve on the boiler is opened, and so much steam diverted directly into the air that it now issues but slowly from the exit tube of the calorimeter.

The balance being let down on its bearings, weights are added till equilibrium obtains. If the substance is quite heated this weight remains absolutely adequate, if not the weight is again adjusted when no further increase is apparent. After the space of about one minute we find the balance continues to vibrate equably; we observe it for some five or ten minutes, and see that there is no change in the equable vibrations of the pointer. We are sure then that the total weight of condensation is obtained. It is found to be 0·798 gram.

A thermometer—also reading to tenths—inserted in a little tubulure provided for it on the boiler, reads 100·25° (corrected at Kew).

The experiment is now completed, and it only remains to make a couple of corrections on the value obtained as the weight of condensed steam before calculating the specific heat of the piece of zinc. Two corrections are necessary—

(1.) The bucket has, by a previous experiment, been found to condense 0·031 gram of water over a range of 87·90°. The range in our experiment is 100·25° − 10·73° = 89·52. Therefore the deduction for the bucket is—

$$0{\cdot}031 + \frac{89{\cdot}52 - 87{\cdot}90}{87{\cdot}90} \times 0{\cdot}031,$$

or 0·0315 very nearly.

(2.) The piece of zinc was counterpoised in air, but the density of steam being less than that of air there was an apparent increase of weight on the replacement of the air by steam, to an amount equivalent to the volume of the substance multiplied by the specific gravity of steam (air being unity) at the pressure prevailing. It is sufficient to take this generally as equal to 0·00062 gram per cubic centimetre displacement (see Appendix). Taking the specific gravity of zinc as 7, and its weight being 48·300 grams, the value of the correction is—

$$\frac{48{\cdot}3}{7} \times 0{\cdot}00062 = 0{\cdot}0043 \text{ gram.}$$

The total deduction from the weight of condensation observed is then 0·0358; we take this as 0·036 in view of the limits of accuracy imposed by the balance. This leaves, as very nearly the true condensation due to the calorific capacity of the piece of zinc raised from t_1 to t_2, the weight 0·762 gram. The quantities are now collected—

$$\begin{aligned} W &= 48{\cdot}300 \\ w &= 0{\cdot}762 \\ t_1 &= 10{\cdot}73^\circ \\ t_2 &= 100{\cdot}25^\circ \\ \lambda &= 536{\cdot}4, \end{aligned}$$

at this temperature (see Appendix). Then by the formula previously given

$$S = \frac{w\lambda}{(t_2 - t_1)W}.$$

$$\text{Specific heat of zinc} = \frac{0{\cdot}762 \times 536{\cdot}4}{(100{\cdot}25 - 10{\cdot}73) \times 48{\cdot}3} = 0{\cdot}09453.$$

Degree of Accuracy attained by the Method.

In a similar way the quantities recorded in the following tables of experiments on metallic elements have been obtained.

Table of Experiments on some Metallic Elements.

Zinc.—Cast and cooled slowly. Free from arsenic, and probably nearly pure.

W.	t_1.	t_2.	w.	Sp. h.	Other observers.
48·300	15·70	99·76	0·714	0·09434	Bède ..... 0·09412
"	15·74	99·80	0·713	0·09421	10° to 100°
"	15·86	99·80	0·713	0·09434	Bunsen ..... 0·0935
"	10·73	100·25	0·762	0·09453	Regnault ..... 0·09555
"	13·80	100·20	0·7345	0·09443	Kopp ..... 0·0932
..	..	..	..	..	Dulong and Petit 0·093
Mean	14°	100°	..	0·09437	

*Aluminium.**—Pure, cast.

W.	t_1.	t_2.	w.	Sp. h.	Other observers.
5·376	16·62	100·18	0·1880	0·2247	Mallet ..... 0·225
„	16·30	100·18	0·1862	0·2215	Regnault ..... 0·2181
„	15·78	100·18	0·1897	0·2242	Kopp..... 0·202
„	16·16	100·18	0·1887	0·2243	
„	16·42	100·18	0·1877	0·2236	
„ ..	16·42	100·18	0·1877	0·2236	
Mean	16°	100°	..	0·2236	

*Silver.**—Pure, rolled.

W.	t_1.	t_2.	w.	Sp. h.	Other observers.
4·027	12·96	99·74	0·0380	0·0583	Regnault ..... 0·05701
„	10·72	99·35	0·0380	0·0571	Kopp ..... 0·056
„ ..	11·44	99·28	0·0375	0·0569	Bunsen ..... 0·0559
Mean	12°	99°	..	0·0574	

Platinum.—Rolled. Purity uncertain.

W.	t_1.	t_2.	w.	Sp. h.	Other observers.
34·764	13·10	99·63	0·182	0·0326	Regnault..... 0·0324
„	13·20	99·63	0·183	0·0327	Kopp ..... 0·0325
„	14·36	99·52	0·186	0·0331	Pouillet ..... 0·0335
„	14·04	99·54	0·183	0·0330	0° to 100°.
„	13·68	99·60	0·183	0·0328	
„	12·80	99·75	0·185	0·0328	
„	14·16	99·82	0·182	0·0328	
Mean....	14°	100°	..	0·0328	

Lead.—Pure, cast.

W.	t_1.	t_2.	w.	Sp. h.	Other observers.
89·508	16·70	99·85	0·428	0·03091	Regnault..... 0·0307
„	16·34	100·04	0·431	0·03093	„ ..... 0·0314
..	..	..	..	..	Kopp ..... 0·0315
Mean....	16°	100°	..	0·03092	

* Received from Professor Reynolds.

Some diversity of result exists among the determinations by other experimenters. Probably much of this diversity is to be ascribed to actual difference in the molecular condition of the samples operated on. The veracity of the present method must be judged then by general agreement with previous observations. In the case of *zinc*, however, the most reliable observations are probably those of Bède, Bunsen, and Regnault. The first of these observers, it will be remembered, extended his observations to the variations in the specific heat of zinc with change of temperature limit.* From those observations the value for the mean calorific capacity between 10° and 100° is deduced as 0·09412. This is identical, it may be said, with the value found by the method of condensation.

Compare, also, in this list of experiments on zinc—and throughout the table—the variations of temperature interval, sufficiently revealed in the varying values of t_1°, with the delicate compensating variations in the weights of condensed vapour, resulting in the close agreement of the values deduced. It will be seen too that among the five results on zinc the extreme variation from the mean amounts to no more than 0·17 per cent.

The experiments on aluminium, silver, and platinum tax more severely the consistency of the experiments, owing to the limits of accuracy imposed by the balance. Of the experiments made on *aluminium*, one is not recorded owing to a doubt about the accuracy of the initial temperature coupled with an abnormal result. The mean finally arrived at agrees with Mallet's experiments† effected in Bunsen's ice calorimeter. This value accords better with Dulong and Petit's generalisation than the lower values of Regnault and Kopp.

In the experiments on *silver*, much care had to be taken in estimating the value of w. No bucket was used, the specimen being looped in a fine platinum wire, the small quantity of water precipitated adhering to its surface. It is the smallest condensation I have attempted to deal with.

The series on *platinum* were some of the earliest effected by the method, and that in a very rude form of the apparatus. Two of the first experiments are not included in the list.

As, I think, some import may fairly be attached to consistency in experiments where prevailing conditions vary, I quote for comparison the extremes of Regnault's experiments on metals I have dealt with.

Platinum	569 grams	7 experiments..	0·03223—0·03279
Zinc	247 „	3 „ ..	0·09528—0·09589
Silver	345 „	5 „ ..	0·05679—0·05739
Lead.....	526 „	3 „ ..	0·03129—0·03150

* 'Mém. Couronnés de l'Acad. de Bruxelles,' vol. 27, 1856.

† 'Chemical News,' vol. 46, 1882, p. 178.

If these be compared with the extremes contained in the table it will be found that, except in the case of silver, the comparison is favourable to the method of condensation.

At the suggestion of Professor Fitzgerald, experiments on distilled water sealed in glass envelopes were undertaken. It appeared probable that the specific heat of this substance, as obtained in experiments made on a very large scale by Regnault, would afford more reliable data for comparison than metallic solids, in the case of which too it is likely that differences in molecular freedom may exist affecting the calorific capacity to a considerable extent. Again it was easy to obtain this substance sufficiently pure with facility. The experiments were carried out as follows:—

A very thin glass bulb is blown before the blowpipe. Its capacity is to be some 15 or 20 c.c. In drawing off the tube, a little hook is left for conveniently suspending the bulb in the calorimeter. It is sealed by closing the extremity of the hook; but before doing so we see that no appreciable quantity of moisture is enclosed, introduced from the breath in blowing it. If moisture is visible it is expelled by repeated heating and cooling, the cooling finally being effected several times in a desiccator. Neglect of this caused appreciable error in some of my experiments; it was ultimately detected in the abnormal specific heat ascribed to glass by the initial experiments on the envelope. It is probable that this trace of water, as suggested by Professor Fitzgerald, and contrary to my original idea when ignoring it, acts more by its latent heat of vaporisation than by its specific heat.

To ascertain the amount of precipitation to be allowed for this envelope, it is hung from a wire loop between the stirrups of the carrier, so that condensation falling from it is received in the bucket beneath.

The total condensation for bulb and carrier together with that due to the air contained in the bulb, including also the effect of displacement difference from air to steam, afford—

(1.)	$w_1=0{\cdot}116$.	....	Range, 11·0—100·35°
(2.)	$w_1=0{\cdot}116$.	....	Range, 11·3—100·35°

The bulb is now opened by removing the extremity of the glass hook, and weighed = 2·084 grams. The height of the barometer (760) and temperature (11°) are noted.

The next operation is to fill it with distilled water. This is effected under the receiver of the air-pump by exhausting while the orifice of the bulb is submerged, and again cautiously exposing to atmospheric pressure. A space is left unfilled to allow for subsequent expansion. The contents of the bulb are now raised to boiling point by immersing in a bath of boiling water, and it is sealed with the blowpipe while at this temperature. This is necessitated by the thinness of the

glass; it was found that such bulbs will not stand the internal pressure otherwise obtaining at steam temperature.

Its weight is now 15·930 grams. It is estimated that its content is altogether 16·5 c.c.; this quantity of air at 760 mm. and 11°, as above, weighs very closely 0·020 gram. The deduction for the weight of the glass envelope in order to deduce the weight of contained water is, then, 2·084—0·020 affording W=13·866.

In subsequent experiments with this bulb of water, it is necessary from the precipitation obtained to deduct w, as ascertained by experiments (1) and (2), above, diminished however by the quantity of precipitation to be ascribed to the calorific capacity of the contained air present in those experiments. Taking the specific heat of air as 0·2389, this retrenchment calculates to be nearly 1 mgrm. of precipitation, and it is taken as such as it probably contained still some moisture. This leaves the deduction as 0·115 for a range of 89·2°. Although the nearly constant displacement difference of the bulb from air to steam is here included, this quantity may without sensible error be recalculated for variations in temperature range as the correction on the large condensations subsequently obtained. The further process of experiment is the same as previously described in the case of zinc.

In this way, the six experiments contained in the following table have been carried out:—

Table of Experiments on Distilled Water.

No.	W.	t_1°.	t_2°.	λ.	w.	Sp. heat.
1	13·866	11·20	99·90	536·56	2·3125	1·0087
2	,,	11·70	99·45	536·90	2·2760	1·0044
3	,,	8·80	99·55	536·80	2·3610	1·0074
4	,,	9·70	99·50	536·80	2·3320	1·0055
5	,,	9·47	100·00	536·50	2·3530	1·0055
6	,,	8·80	100·10	536·40	2·3740	1·0059
	Mean ..	10°	100°	..	..	1·0062

Between same limits, according to Regnault, specific heat = 1·0055.

The last four experiments in the table were carried out in such a way that the rate of precipitation was much retarded, in fact the conditions were altered to those prevailing in the case of a very badly conducting body. This was effected by shielding the glass bulb from direct contact with the steam by a loose fitting copper-foil box, an intervening air-jacket being left around the bulb. With this arrangement precipitation was hardly completed in 15 minutes; the naked

bulb was quite heated in something less than four. A deduction was of course made for the shield by preliminary experiments.

I would point out that any error in Regnault's estimate of the increase of specific heat of water with rise of temperature will militate in a double sense against the agreement of his results with mine. For this increase is assumed by Regnault in deducing, from his formula for the total heat of steam, the heat of evaporation, and, as it is subtractive, an error on the side of excess in his estimate of the specific heat of water introduces an error on the side of insufficiency in the value ascribed to λ, and *vice versâ*. Thus not alone would the quantity with which I compare my results be erroneously assumed, but in proportion as error exists in its value, error in the opposite direction would be introduced into my results.

I add some experiments bearing on questions of radiation error as affected by extent and nature of surface exposed to the steam. They are from those made on minerals, and are embodied in a paper on the specific heats of minerals. ('Roy. Soc. Proc.,' vol, 41, p. 250.)

The following table contains cases where experiments were re-

Table I.

	W.	w.	$t_1°$.	$t_2°$.	Sp. heat.
Limpid crystal of *Barites*	76·109	1·405	9·65	100·30	0·10923
A portion of the same in small fragments	65·143	1·195	9·60	99·80	0·10910
Rhombohedron of *Iceland spar*	33·904	1·156	10·00	99·76	0·20383
Same in small fragments.....	33·059	1·079	13·34	99·50	0·20345
Specimen of *Talc* in one piece..	47·445	1·685	12·32	100·25	0·21671
Two fragments from same hand-specimen.....	44·631	1·568	13·30	100·20	0·21686
Specimen of *Lepidolite* — one piece	42·726	1·470	12·20	100·20	0·20978
Two pieces from same hand-specimen.....	48·823	1·676	12·38	100·20	0·20967
Five crystals of *Amphibole*	57·192	1·835	12·00	99·72	0·19634
The two largest of the five	45·133	1·443	12·94	100·30	0·19635
The three largest of the five ...	51·309	1·640	12·90	100·30	0·19619
Fragments of milk-white *Oligoclase*	48·074	1·546	12·60	99·10	0·19974
Pieces from same hand-specimen	41·829	1·366	11·60	99·40	0·19967
Flakes of *Biotite*	20·291	0·685	12·63	100·30	0·20651
Part of same.....	17·809	0·593	12·70	99·95	0 20491
Crystal of *Muscovite*	20·541	0·687	12·40	99·90	0·20519
Loose flakes from same hand-specimen.....	23·735	0·778	13·80	99·75	0·20466

peated under very different conditions of surface area. It is evident from them that the extent of surface exposed by a substance to the steam affects the result only in a very trifling degree; the repeated consistency can hardly be the result of chance. As liability to radiation errors will increase, *cæteris paribus*, with extent of surface, they reveal, I think, that the method is not open to appreciable error from such sources.

Table II illustrates, in the cases of gypsum, calcite, and aragonite, minerals fairly constant in chemical composition, fidelity to the chemical nature of a substance through varying conditions of weight, bulk, shape, and surface texture.

Table II.

	W.	*w*.	$t_1°$.	$t_2°$.	Sp. heat.
Hydrous calcium sulphate. *Gypsum* :—					
Clear crystal of selenite	21·048	0·965	10·00	100·10	0·27264
Four clear crystals of selenite	13·204	0·623	7·24	100·40	0·27164
Fibrous silky gypsum	30·150	1·393	9·20	100·40	0·27167
Fragments of rough white gypsum	31·547	1·456	9·98	100·40	0·27374
Calcium carbonate — *Calcite*, rhombohedral :—					
Iceland spar, limpid crystal..	33·904	1·156	10·00	99·76	0·20383
Milk-white rhombohedron...	35·815	1·135	16·75	99·93	0·20439
Three slender hexagonal crystals, limpid	10·044	0·329	13·04	99·60	0·20340
Group of tapering, clouded prisms	31·524	1·075	11·90	99·50	0·20908*
White chalk, in fragments...	22·454	0·747	12·30	99·80	0·20415
White chalk, fragments from same specimen	21·737	0·706	13·77	99·35	0·20390
Calcium carbonate—*Aragonite*, orthorhombic :—					
Two yellow transparent crystals	24·331	0·806	12·45	99·65	0·20401
Two limpid crystals	28·161	0·934	11·60	99·00	0·20393

Apart from experiment, I think sufficiently careful consideration shows that we might expect very considerable accuracy from the method. There is theoretically, it will be found, but one source of error needing serious consideration; it is the danger of precipitation occurring, not on the surface of the substance, but at points close to

* For a brief notice of such abnormal cases see my paper "On the Specific Heats of Minerals," 'Roy. Soc. Proc.,' vol. 41, p. 250.

the surface; in other words, the formation of mist by radiation from the steam to the cold substance.

Now it is assured that on the replacement of air by steam a film of water is instantaneously deposited on the surface of the substance, and, assuming an abundant supply of steam, condensation then proceeds as fast as heat is transferred by conductivity and diffusion across this film to the cold surface of the solid beneath. While then we must assume that the inner surface of the film is appreciably at the temperature of this cold surface, it is probable that we may also assume the outer surface of the film to be throughout the experiment nearly at the temperature of the steam. There can be but little difference, for fall in temperature is made good with great rapidity by fresh condensation. There is here in the first place a favourable condition, as surface radiation can, under such circumstances, be but insignificant in amount, and radiation from points in the surrounding steam can only be supposed as affecting points within the film or, in some cases, within the substance. Only in this way can we suppose the formation of precipitation elsewhere than at the very surface of the film by actual contact. But again, the opacity of water to radiation from aqueous vapour will here afford protection. This opacity, it will be remembered, has been shown by Tyndall and others to be very great. Thus I find that Tyndall records among his experiments that a layer of water but 0·07 of an inch in thickness transmitted but 1·1 per cent. of the radiation from a hydrogen flame. It may be supposed that the more perfect accord between the vibration periods of radiator and absorber obtaining in the calorimeter will ensure greater, if not perfect, opacity.

The contact precipitation, consequent on the loss of *vis viva* of the impinging molecules—the film transmitting the energy to the substance—insures that there will be many molecules entering and few leaving, and hence extension of free path or diminution of pressure in directions normal to the surface of the substance. There is then superadded as a protective element a converging drift of steam upon the substance, tending to restore to it what precipitation may possibly be due to radiation. Under perfect conditions of steam supply—could we suppose, for example, the substance in a chamber which could be supplied with steam from so many points that we might substitute the idea of an unlimited region of steam suddenly brought around it—this drift, I think, excludes almost the possibility of error supposing radiation. In attainable conditions, where cross draughts will prevail to some extent, there would still evidently be protection involved in the very fact of condensation.

There are, then, three circumstances tending to nullify this error; the high temperature of the external surface, the adiathermanous nature of the water film, and the converging drift of steam. Whether

all three act, or whether the first two are not practically efficacious of themselves, I think that theory is here in accord with experiment in showing that on this count no appreciable inaccuracy is to be apprehended.

There are a few other considerations. From the facts that there is a fall of temperature across the film, the gradient descending nearly to the temperature of the substance on the one side, and that throughout the period of condensation the film continually gravitates downwards as it receives accretion from without, it might be expected that water which had not yet risen to steam temperature might in some cases reach the bucket. This occurs, but need obviously be no source of error. If experiments be made in glass calorimeters, the formation of a dew on the underside of the bucket will often be noticed; to this extent it does no harm. But in the case of sudden and copious precipitation, especially in the case of smooth substances, where water runs off more freely at points close to the surface of the body, the precipitation on the outside of the bucket may collect in such quantity that drops fall off. I have occasionally noticed this, and it should be borne in mind; but with the flat form of bucket used, and the safeguard afforded by the supporting claw, there is little danger of this occurring up to rapid precipitations not much exceeding a couple of grams. Over that amount a second catchwater, which may be of small dimensions, should be provided beneath the claw.

The substance hanging in the steam is safe from radiation errors. The phenomena attending accretion of weight terminate in the conditions which prevail in the case of a liquid in presence of a saturated atmosphere of its own vapour, the whole maintained at constant temperature and pressure, when the relative weights of liquid and vapour remain constant statistically, and absolutely so far as we can measure. If radiation from the hot substance to the cooler walls of the calorimeter occurred, constancy of weight would not be attained. It is certain that the least radiation across the steam-jacket would be revealed in continued slow accretion of weight. But there is nothing so striking in experiments effected by this method as the steadily maintained equilibrium of the balance once the substance has attained the temperature of the steam, the pointer vibrating with perfect equality so long, apparently, as we chose to prolong the experiment. If the steam supply be cut off for a moment there is loss of weight, and on restoration of steam there is finally on the whole gain of weight, for radiation, as well as evaporation, occurs in the absence of steam. If the pressure be appreciably increased by throttling the exit pipe and partially closing the boiler for a couple of seconds, the balance reveals increase of weight; but so long as the pressure is neither increased nor diminished equilibrium is absolutely preserved.

It is obvious that disturbing effect due to water-dust or mechanically

suspended water in the steam is negatived by this constancy of weight. Probably such dust is all deposited in the steam pipe shortly after leaving the boiler.

Mist or water-dust doubtless is present at first, due to the condensation produced in heating up air in the calorimeter. Some of this is thrown upon the substance, but this will be small in quantity, probably inappreciable, and acts against what error is due to radiation from the incoming steam to the substance. This last is an error analogous to the error of transference in other methods, but with easily attainable rapidity of air displacement probably far less.

The degree of accuracy attainable in effecting the measurements remains to be considered. Of these, it is evident that in obtaining the temperature limits great accuracy is attained by the use of thermometers of no extraordinary sensitiveness. If t_1° and t_2° are reliable to $\frac{1}{10}$ part of a degree, over a range of 80° we are observing to $\frac{1}{800}$ part the quantity to be measured.

As regards the observation of w, the weight of water condensed, obviously the degree of accuracy attainable depends on the sensitiveness of the balance and the magnitude of w. It is also necessary, however, to consider in some detail how far it has been found possible to perfect the mechanical conditions attending the observation of the weight of the substance while immersed in steam.

It is evident in the first place that as regards the danger of introducing error into the equilibrium of the balance by heat radiating to its parts, the difficulty may at once be got over by arranging things so that the calorimeter is outside the balance case, as beneath it, the balance standing on a shelf, suppose, and a wire taken down through apertures in balance floor and shelf. In the form already described, indeed, I have detected no error due to unequal heating of the beam, although I have frequently looked for it by unhooking the substance during experiment and testing for want of equilibrium. This is due to the protection afforded by the non-conducting material of the tunnel. It was necessary, however, to retain but one pan, as heat conducted by the rails through the floor of the calorimeter set up a draught tending to draw up the pan.

I have detected no error, measurable by the balance, due to condensation on the wire for suspending the substance, where it issues from the calorimeter. If this wire be closely observed it will be seen that a little bead of water forms on it just above the mouth of the aperture, and although occasionally reinforced by another little bead slipping down to it, it soon reverts to its original bulk, probably by evaporation promoted by heat conducted along the wire. With the size of wire used—about 0·15 mm. in diameter—I do not think this bead of water ever exceeds half a milligram in weight, and, as I say, the balance shows no sign of any gradual increment of weight. This

experiment on the constancy of weight has been extended to over half an hour on many occasions.

The use of plaster of Paris in forming the aperture avoids what uncertainty and want of sensibility would be introduced into the weighing by water permitted to accumulate in the orifice. Until this plan of cutting the aperture in a bibulous material was resorted to, the accumulation of water gave much trouble. The plaster remains dry when in use for an indefinite time, and wears well. That now in the calorimeter has served through more than 300 experiments. It is also very easily renewed.

In order to avoid as far as possible escape of steam up the wire and ensure constancy of pressure, I have lately adopted a plan which acts automatically in keeping the calorimeter full of steam sensibly at atmospheric pressure. The arrangement consists in locating the escape valve of the boiler at a level so much below the level of admission into the calorimeter, that a small and constant pressure—due to the levity of the steam—is established, urging the gas to ascend the tube leading to the calorimeter. This end is secured by affixing the escape-valve to the end of a wide tube which is taken from the boiler bent twice at right angles and carried down to a level outside the boiler sufficient to secure the requisite pressure; a couple of decimetres "head" will in general be sufficient.

The effect on the weighing of steam draught within the calorimeter can be entirely avoided by shielding or jacketing. In the form already described, indeed, it seldom gave trouble, but in this respect I have in a recent form improved the apparatus. It is worthy of notice, also, that the conditions prevailing when we weigh a substance at a temperature different from that of the air within the balance case do not obtain here, the substance in the calorimeter being surrounded by an atmosphere at its own temperature.

Description of an Improved Gravimetric Calorimeter.

I have designed an apparatus intended to carry out such conditions as have suggested themselves as conducing to accuracy and to ready working. Its advantages have already to a great extent been practically tested in a recently completed instrument, but this instrument serving itself to suggest improvements is not the same as that now described. Plate 7 shows a vertical section and plan.

The apparatus it will be seen consists of a cylindrical chamber with conical ends. This chamber is divided in a vertical plane passing through its axis, the separate halves being firmly hinged on a tripod borne on three adjusting screws. The chamber is thus readily opened by turning back the halves, and closed by pressing them together, when they meet on a ground edge. It is made of brass and kept as

slight as possible. The half chambers being closed, steam is admitted at the top of the apparatus, the expelled air escaping at the bottom through a tube which passes through the base of the instrument. In plan it will be seen that the steam is admitted by a forked or branching tube at two tubulures, which in elevation are seen to be cut back so as to afford a ready seat for the supply pipe, and also that the latter when placed in position may conduce to bind closer the half-chambers. The steam pipe is supported further and retained in position on a light pillar springing from the tripod base. To its seat on this pillar it is clipped by a spring bearing against the centre of the fork, but so that it may readily be laid in its position. A little tubulure is also provided for conveniently taking the temperature by inserting a thermometer.* At other times this tubulure is closed by a little cap (not shown in the drawing). Two half-cylindrical plugs of plaster of Paris, notched at their meeting faces, serve as before to pass the wire depending from the balance, which is supposed to rest upon a shelf placed above the apparatus.

Within the outer shell of the calorimeter an inner shield of very slight brass is provided, similar in shape, and capable of a rotational motion of about 60° about its vertical axis. This is effected by attaching it to a tube which rotates smoothly in the exit-tube at the base of the calorimeter. It is worked by a stud projecting through a horizontal slot cut in the latter. The shield is made in halves, and opens on hinges affixed to the rotating tube. Above, each half of the shield is perforated, so that when in one extreme position it receives within it all the entering steam, but on being rotated 60° no longer admits steam directly, the perforations being then turned away from the entrance tubulures.

When it is desired to close the exit-tube of the calorimeter, a stop is pressed in through a horizontal slot in the tube. Water condensed from the steam by radiation from the calorimeter is received in a little trough placed beneath the tripod stand.

The apparatus is worked as follows:—

The carrier being suspended at the right height on the wire from the balance, and the substance being arranged upon it, the calorimeter, with its inner and outer cases drawn back, is moved under and then closed upon it. The inner case is now rotated so that the calorimeter is thus still further closed, and also one of the apertures of the shield, so placed that a thermometer may be passed through the small tubulure into the interior of the calorimeter. To guard the calorimeter against sudden changes of temperature while the thermometer is assuming

* The effect of scale parallax in reading thermometers may conveniently be avoided by bringing the scale into coincidence with its reflection on the mercury thread of the thermometer. The way of doing this is easily acquired after a couple of trials.

the temperature $t_1°$, it is necessary to provide a cover of nonconducting material—as wood—made in halves, and closing so as to permit the reading of the thermometer.

When it is desired to carry out the experiment, the forked tube, which is connected with the boiler by 2 or 3 metres of rubber tubing, is separately heated with steam, the valve on the boiler being closed. The thermometer in the calorimeter is then read and withdrawn, the cover removed, and the shield rotated into the position for admitting steam. The steam-pipe is now laid in position; the construction enables this to be done very rapidly and easily. Steam is thus passed directly into the inner chamber at two points, and the inner chamber being very slight condenses but a small quantity of steam, so that the displacement of the air is retarded as little as possible, and is, in fact, with the empty calorimeter almost immediate. On steam flowing out freely at the exit-tube, the valve in the boiler is opened and the calorimeter closed by pressing home the stop in the exit-tube.

The adjustment of the wire in the orifice is now effected by the screws on the tripod. Effected in this way this is a very easy operation, although the orifice may be hardly 1 mm. in diameter and the wire some 40 cm. long. Balance and calorimeter must, however, be steadily supported. The weighing is now begun, and when it is found that the substance is no longer condensing steam, is finished with the shield in the calorimeter in its position for sheltering the substance from draughts. On this operation being completed, the thermometer for taking $t_2°$ is inserted in its tubulure, which is now again over one of the orifices in the inner shield and hence admits the thermometer freely into the calorimeter.

Appendix.

Corrections for Displacement in Steam.

(1.) The substance being counterpoised in air is not when immersed in steam—apart from the occurrence of condensation—any longer in equilibrium with the counterpoise, the density of saturated steam at ordinary pressures being different from that of air. The deduction from the apparent weight of condensation on the substance is equivalent to the volume of the substance multiplied by the difference in the weights of unit volumes of steam and air. By the apparent weight of condensation is meant that deduced on subtracting from the weight added to the counterpoise during an experiment the weight due to condensation on the carrier. Let this apparent weight be w_1.

The difference in the weights of unit volumes of steam at 100° and air at 15° is 0·000636, calculated on the absolute densities of dry air and steam according to Regnault.

The correction on w_1 is then—

$$-V \times 0{\cdot}000636 \text{ gram},$$

where V is the volume of the substance in cubic centimetres.

(2.) If extreme accuracy be desired, it is necessary further to reduce the weight of condensation to vacuo. Calling w_2 the weight deduced after correction (1), the second correction is on w_2, and is—

$$+w_2 \times 0{\cdot}000589,$$

the factor 0·000589 being the absolute density of saturated steam at 100° (Regnault). It is assumed that we may for the purpose consider unit weight of water at 100° as equal to unit volume.

Finally the true weight of water w is very nearly—

$$w=(w_1-V\times 0{\cdot}000636)\times 1{\cdot}000589.$$

Regarding the factor 0·000636 I may mention that a direct experiment made in the calorimeter afforded 0·00062 as the difference in weight of unit volume of steam at 100·2° and air at 17·7° and 766 mm. The experiment consisted in hanging a heated sphere of blown glass in the calorimeter—it having been previously counterpoised—and admitting steam before the sphere had cooled to steam temperature. When a sufficient time had elapsed to secure equilibrium of temperature the amount necessary to add to the counterpoise to restore balance was ascertained; this divided by the volume of the sphere afforded the relative density sought.* In the experiment the volume of the sphere was 38·7 c.c., the weight added 0·024 gram.

On the Value of λ.

If the value of λ be calculated from Regnault's formula for, say, the cases of a barometer at 720 and 780, the values 537·2 and 536·0 are obtained; at 760 it is 536·5. In careful experiment this variation must be considered. My practice has been to allow for it by the approximate assumption for ordinary variations of pressure that the correction on the value of λ for the standard pressure 760 mm. is—

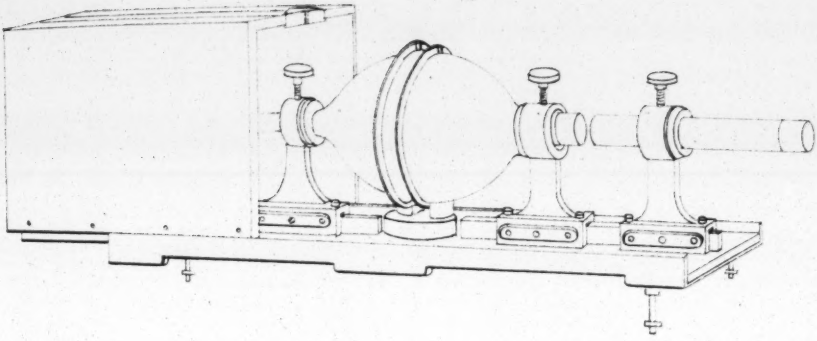
$$+0{\cdot}7(100-t_2^\circ),$$

that is I take

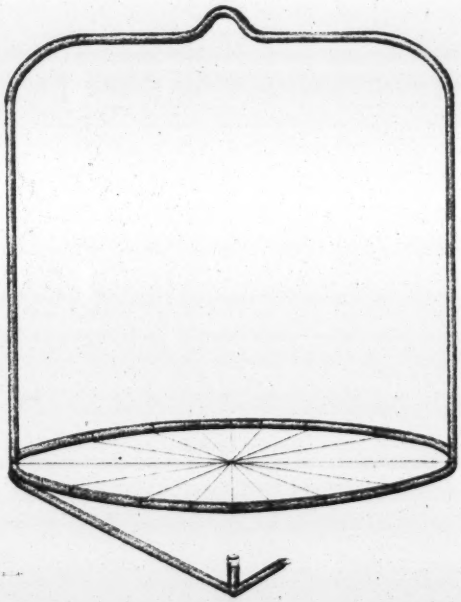
$$\lambda=536{\cdot}5+0{\cdot}7(100-t_2^\circ)$$

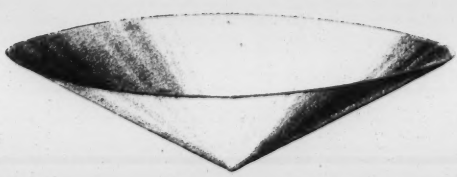
where t_2° is the temperature of the steam, read directly by a thermometer or deduced—as it conveniently may be and with great accuracy—from the height of the barometer and reference to Regnault's tables for the corresponding boiling temperature of water.

* See "On some methods of Measuring the Densities of Gases," by G. F. Fitzgerald, F.R.S., 'Roy. Dublin Soc. Proc.,' vol. 4, 1885, p. 481.



CARRIER AND PAN.

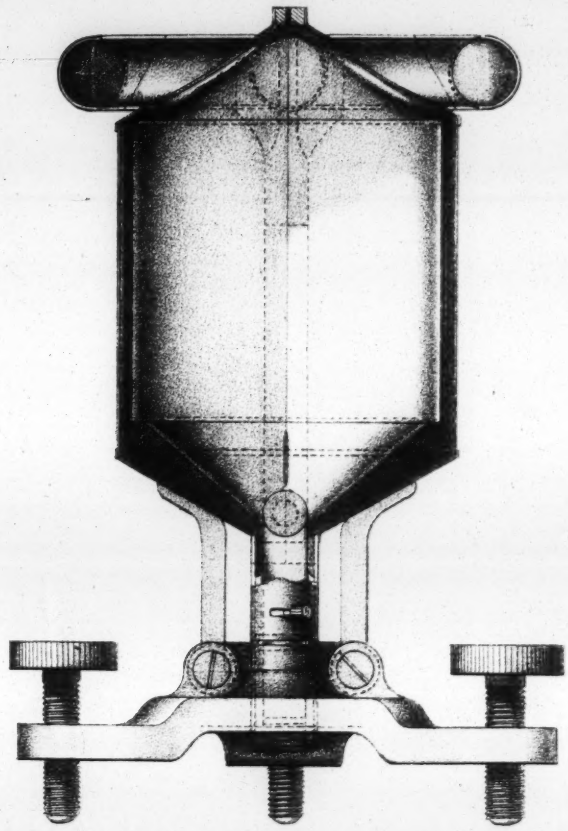




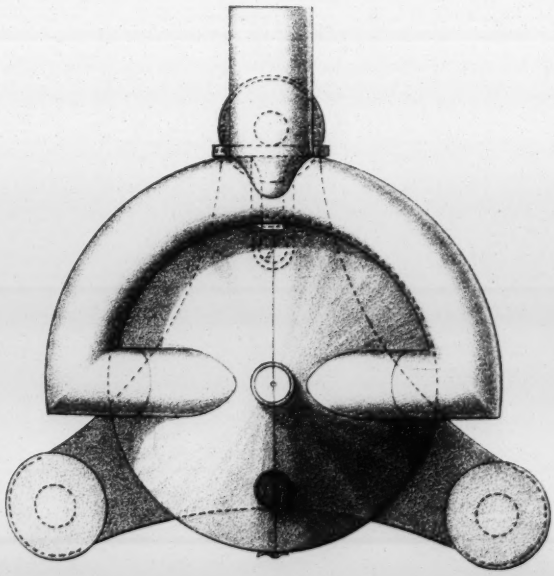
West, Newman & Co. lith.

Proc. Roy. Soc. Vol. 41. Pl. 7.

A GRAVIMETRIC CALORIMETER.



SECTIONAL ELEVATION.



PLAN.

West, Newman & Co. lith.

Complete Formula for Calculating the Specific Heat.

Collecting the foregoing corrections we get a formula for use if very considerable accuracy be desirable, but it is to be observed that under any ordinary circumstances correction (1) on w_1 of $-\mathrm{V}\times\cdot000063$ is sufficient.

The complete formula for S, the specific heat, is—

$$\mathrm{S}=\frac{1\cdot000589[w_1-\mathrm{V}\times 0\cdot000636][536\cdot5+0\cdot7(100-t_2^\circ)]}{\mathrm{W}(t_2^\circ-t_1^\circ)},$$

the several weights observed being supposed reduced to vacuo as usual, otherwise the letters being as before.

November 30, 1886.

ANNIVERSARY MEETING.

Professor STOKES, D.C.L., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Society was presented, by which it appears that the total receipts during the past year, including balances carried from the preceding year, amount to £7,226 8*s.* 9*d.*, on the General Account, and £10,107 7*s.* 1*d.* on account of Trust Funds, and that the total expenditure in the same period, including purchase of stock, amounts to £5,363 12*s.* 11½*d.*, on the General Account, and £6,911 16*s.* 6*d.*, on account of Trust Funds, leaving a balance on the General Account of £1,838 5*s.* 10*d.* at the Bankers', and £24 9*s.* 11½*d.* in the hands of the Treasurer, and on account of Trust Funds a balance at the Bankers' of £3,195 10*s.* 7*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Nov. 30, 1885).

On the Home List.

Apjohn, James, M.D.
Boileau, John Theophilus, Major-General, R.E., F.R.A.S.
Bunbury, Sir Charles James Fox, Bart., F.G.S.
Busk, George, F.L.S.
Cardwell, Edward, Viscount, F.G.S.
Chapman, Thomas, F.S.A.
Cobbold, T. Spencer, M.D.
Enniskillen, William Willoughby, Earl of, D.C.L.
Evans, Sir Frederick J. Owen, K.C.B., R.N.
Fergusson, James, C.I.E., D.C.L.
Forster, Right Hon. William Edward.
Guthrie, Professor Frederick.
Shadwell, Sir Charles Frederick Alexander, Vice-Admiral, K.C.B.
Solly, Edward, F.S.A.
Strafford, George Stevens Byng, Earl of.
Thomas, Edward, C.I.E., F.S.A.
Waveney, Robert Alexander Shafto Adair, Lord.

Withdrawn.

Bouverie, Right Hon. Edward Pleydell.

Fellows elected since the last Anniversary.

Bidwell, Shelford, M.A.
Colenso, William, F.L.S.
Dixon, Harold B., F.C.S.
Festing, Edward Robert, Major-General, R.E.
Forsyth, Andrew Russell, M.A.
Green, Professor A. H., M.A.
Horsley, Prof. Victor, F.R.C.S.
Meldola, Raphael, F.R.A.S.
Pye-Smith, Philip H., M.D.
Rosebery, Right Hon. Archibald Philip Primrose, Earl of.
Russell, Henry Chamberlaine, B.A.
Sedgwick, Adam, M.A.
Thurlow, Right Hon. Thomas John Hovell-Thurlow Cumming-Bruce, Lord.
Unwin, Professor W. Cawthorne, B.Sc.
Warington, Robert, F.C.S.
Wharton, William James Lloyd, Captain R.N.
Wilde, Henry.

On the Foreign List.

Baeyer, Adolf.
Klein, Felix.
Kowalewski, A.
Lovén, Sven.

The President then addressed the Society as follows:—

For many years it has been my duty as senior Secretary to read at each Anniversary the death-roll of the year. The names this year are perhaps slightly fewer than usual, but many recall to us faces once familiar that we shall never see here again. Earliest among them comes Sir Frederick Evans, whose death took place only very shortly after our last Anniversary. In the course of the preceding summer he crossed the Atlantic to take part in that International Conference which assembled at Washington, to deliberate among other things on the choice of a common prime meridian for all civilised nations. On his return he was looking ill, and the illness increased until it carried him away. Yet even through his illness he kept on working at science, at a task he had undertaken, and which was almost completed when he died. To this I shall have occasion to refer again. In Mr. Busk we have lost one whose detailed knowledge of certain branches of natural history and comparative anatomy was almost unrivalled. He took an active part in the scientific business of the Society, and repeatedly served on our Council, and both then and subsequently gave us the benefit of his extensive knowledge and sound judgment in the important but laborious task of reporting on papers. In Lord Cardwell we have lost a statesman whose political duties did not prevent him from coming among us and serving on our Council. The public services and singular honesty and straightforwardness of Mr. Forster are appreciated by the nation at large. Quite recently, at no advanced age,

we have lost Professor Guthrie, the occupant of a chair which a great many years ago I held for a time; a man whose genial character drew around him a close circle of friends. Still more recently we have lost the Earl of Enniskillen, whose fine palæontological collections are well known to geologists. Only the other day one passed away whom we seldom missed at our anniversary meeting, and who was frequently with us on other occasions: I allude to General Boileau, whose philanthropic labours will not soon be forgotten, and may, I trust, be recognised in a much needed form.

The Fellows will have noticed with satisfaction a very considerable excess of income over expenditure in the balance sheet for the year. At first sight it might be supposed that as the 'Transactions' come out at irregular intervals there might have been fewer parts published than usual; but it will be found on examination that the past year has borne its proper share of printing expenses. The excess is really due to a substantial improvement in the Society's property, under the careful and judicious management of our Treasurer.

Last year our President informed the Fellows of the munificent offer made to the Society by Sir William Armstrong to give to the Scientific Relief Fund the sum of 6,500*l*., provided an equal sum were raised within a year by subscription from the Fellows, and if need be, other friends of science who might not belong to the Society. As the Fellows are aware, a circular was sent round by the Treasurer mentioning Sir William Armstrong's generous offer, and inviting subscriptions; and the Treasurer has also written privately to a number of persons, Fellows, and others. The sum subscribed or promised in response to this invitation amounts to about 4,200*l*.; and though the sum thus raised does not amount to what Sir William promised to duplicate if it could be raised, he has most generously not only waived the non-fulfilment of the condition subject to which the former offer was made, but has still further augmented it; and he now promises not only to duplicate the sum raised in answer to the Treasurer's appeal, but to give the further sum of 3,600*l*., thus raising the capital from the present sum of about 8,000*l*. to 20,000*l*. He will be ready to pay the whole sum of 7,800*l*. as soon as the subscriptions promised to the Treasurer have been collected. The only condition attached to this princely gift is, that besides meeting the ordinary objects for which the Fund was instituted, the Council should feel themselves at liberty to apply a portion of the income towards the remission of fees in cases of urgent necessity.

The path of the total eclipse of the 9th of September last year, in any place where it fell on land, was so remote from this country that no expedition went out to observe it. It was visible in New Zealand, and in anticipation of it our Eclipse Committee sent out a memorandum to the Colony indicating the points of special interest to look

out for. We have received accounts, drawings, and photographs of the eclipse from Dr. Hector and others. One of the most remarkable features of this particular eclipse was the appearance of two white and unusually bright prominences which attracted general notice, and were compared to electric lamps, and which, situated on opposite sides of the sun, were just over the places of two large spots, one close to the limb, and on the point of disappearing, the other not seen before the eclipse but visible next day, having in the meantime come round the limb.

The present year afforded another of those rather rare occasions, always of brief duration, which are afforded for the study of solar physics by a total eclipse of the sun. Calculations made long beforehand showed that a total eclipse was to be expected on the 29th of August. The path of the total phase on the earth's surface is always narrow, say 100 miles or so across, and on this occasion it swept obliquely across the Atlantic Ocean, cutting the Western Coast of Africa about Benguela, and sweeping across some of the West India Islands to a part of the mainland of South America, where it ended.

Though there was a long belt of ocean over which the totality would be visible, and where the imposing spectacle of a total eclipse might be witnessed, this was not available for regular scientific observations, which require land on which to fix the instruments. On the mainland of America the total phase would come on so shortly after sunrise that the sun would be too low for good observations, and therefore the Island of Grenada, which lay within the belt of totality, was much to be preferred.

Of the two available stations, one lay in the British dominions, and was pretty easy of access from England, and accordingly it seemed to be the duty of our country to take a foremost place in the observations, the results of which would be available to the whole scientific world. It was contemplated first to send expeditions to both places—to Benguela as well as Grenada. The cost of this would, however, exceed what could be spared from the Government Grant without unduly curtailing what was available for other objects. Accordingly application was made to the Lords of the Treasury for a grant of 1,000*l.* towards the cost of the expeditions. Enquiries were also made as to the probable climate at the two places; and here I have to express our thanks to the Governor-General of the Windward Islands, who put us in communication with Dr. Wells of Grenada, from whom we obtained valuable information regarding the climate of that island, and to the Consul-General for Portugal, who obtained information for us from the Polytechnic Institution at Lisbon as to the amount of sunshine about the end of August at Loanda, which may be taken pretty well as representing Benguela.

The information we obtained from various sources as to Benguela

was rather conflicting, but there seemed a pretty general agreement that even if the sun should be shining at the time of the eclipse the sky was likely to be hazy. This would much interfere with good observations, especially as regards the corona; and as the expense of the expedition to Benguela would be considerable, and the success very doubtful, we thought it better to give up that part of the project and confine ourselves to Grenada. Being anxious to trench as little as possible on the national expenditure, and finding that a little more could be taken from the Government Grant than we had expected, we wrote to the Treasury reducing our application to 500*l*., which with assistance from the Admiralty in the shape of the use of a ship-of-war on the West Indian station, and supplemented by some money from the Government Grant and from our own Donation Fund, might enable us to meet the expenditure.

The result was that a sum not exceeding 500*l*., to supplement what could be spared from the Government Grant, was granted, and the expedition, as the Fellows are aware, has sailed and returned. It was fairly successful, the observations having been prevented by clouds at only one of the stations occupied.

There has not yet been time to discuss the observations in full, but two points already appear to have come out pretty clearly. One is that the brightness of the corona, which on this occasion was actually measured, was much less than had been expected, and less apparently than it had been on former occasions. This seems to show that the brightness is liable to great changes when we compare different years, as we know is the case with the form. The other point touches on the question of the possibility of photographing the corona independently of an eclipse. If the photographic brightness of the corona be not overpowered by that of the atmospheric glare immediately around the sun when there is no eclipse, then when the sun is partially eclipsed we might expect to be able to trace the outline of the limb of the moon for some way outside the sun, since the moon would be projected on the background of the corona. The experiment was tried both by Captain Darwin at Grenada, and by Dr. Gill at the Cape, but in neither case was the limb traceable outside the sun. This throws doubt on, but does not disprove, the validity of the method proposed by Dr. Huggins; for he himself has never obtained photographic appearances apparently referable to the corona since the Krakatoa eruption. It may be that the finely suspended particles, whether connected with the Krakatoa eruption or not, which produced those gorgeous sunsets that were so remarkable, have not yet wholly subsided, and cause a considerably increased atmospheric glare. It may be that the corona has actually been much less bright than usual for the last few years.

The present year has been signalised by that remarkable volcanic

explosion in New Zealand, of which we have read accounts in the newspapers. We have received from Dr. Hector a series of photographs of the district, taken at no great length of time after the explosion.

The Krakatoa Committee, which was appointed at the suggestion of our late President to collect information relative to the great eruption, have now I may say completed their work. The Royal Meteorological Society had appointed a Committee to get together information respecting the remarkable atmospheric phenomena witnessed after the eruption. It was thought desirable that the two Committees should work in concert, and accordingly our Committee was enlarged by the addition of two members of the Royal Meteorological Society, even though they did not happen to be members of the Royal Society, who undertook that share of the work. The information collected under this head is naturally voluminous, since it requires no special training to describe the atmospheric appearances. Our late Fellow Sir Frederick Evans undertook the sea disturbance, and continued to work at it even in an advanced stage of the disease which carried him off. Another fortnight, it was estimated, would have enabled him to complete it. His account was found to have been written in pencil on separate sheets of note paper, but his successor in the office of hydrographer, Captain Wharton, our Fellow, was so good as to take up the work; and partly by the use of materials left by Sir F. Evans, partly by his own independent labour, he has now completed it. The report on air disturbance was undertaken by General Strachey, and is ready. Professor Judd undertook the geological part; the materials are ready, and though the actual report is not yet written, the writing would take but very little time. Mr. Scott undertook to collect information as to floating pumice; but as it has been found that the Krakatoa pumice does not possess distinctive features whereby it could be recognised, and therefore the origin of the pumice that ships have encountered at a distance from Krakatoa remains unknown, little trustworthy information could be obtained under this head, and the report has been handed over to Professor Judd to embody with the geology. The heaviest part of the report, that relating to sunsets and atmospheric phenomena, has been prepared by the Hon. Rollo Russell and Professor Archibald, the two Fellows of the Royal Meteorological Society who have been mentioned as having been added to the Committee, and is ready, with the exception of a little revision, and it remains only to prepare an introduction, index, &c. The whole report may therefore be regarded as all but complete in manuscript, and it will be for the new Council to deal with it.

The Circumpolar Committee have now brought their labours to a close, the report on the observations taken by Captain Dawson

at Fort Rae being printed and published. The reports of the expeditions undertaken by Austria, Germany, and the United States of America, are, I believe, complete, and those by France, Holland, and Russia are in a forward state. Before the accounts of the observations taken at different stations by the observers of different nations shall have been for some time before the public, it would be premature to expect general conclusions to be deduced from this great undertaking.

Very satisfactory progress has been made during the past year with the publication of the Report of the "Challenger" Expedition. The volumes already published and in the Society's Library now amount to sixteen on Zoology, and three introductory on other subjects. Others are in a very forward state, and it is expected that the whole will be published very nearly within the time mentioned by the Committee, probably by the end of the next financial year.

As mentioned in the Presidential Address last year, advantage has been taken of the British occupation of Egypt to make some explorations by way of boring in the Delta of the Nile, to the results of which geologists attach great importance. The War Department has allowed some of the staff of the Royal Engineers, when their services were not otherwise required, to take part in the operations, and has lent the boring apparatus, and the Royal Society voted the sum of 350*l.* out of its own Donation Fund to defray the cost of labour and other incidental expenses. It was contemplated originally to make a chain of borings, but the depth to which it has been found necessary to proceed in order to get through the ordinary deposit has turned out to be so great that it was thought better, instead of attempting many, to try and get if possible down to rock, or to something else which might afford evidence that what could be referred to alluvium from the Nile or drifted sand had really been got through. A deep boring has accordingly been made at Zagazig, under the direction of Captain Dickenson, R.E. This has now been carried to a depth of 190 feet 6 inches below the surface, or 164 feet 5 inches below the mean sea-level at Alexandria, and yet nothing has been reached but sand and clay with small pebbles. Professor Judd is now engaged in the examination of the matter brought up. A derangement of the boring apparatus prevented for the present further progress, and the use of a narrower pipe than any at hand would be required for carrying the boring deeper. The Committee considered that it would be more important to extend this boring, so as if possible to get down to rock, or else to some deposit with fossils, than to make a fresh boring in a different place, and arrangements are being made accordingly. The inquiry was deemed a proper one to be assisted out of the Government Grant, and the sum of 200*l.* has been voted from this source to supplement the Royal Society's grant already mentioned.

The ordinary meetings of the Society are well known, and are frequently attended by strangers by permission of the Fellows present; and the papers brought before us are known to the world through our publications. But a great deal of scientific work is done of which the outside public know nothing. There have been thirteen meetings of the Council during the year, and the attendance at our Council meetings is remarkably good. There have been more than seventy meetings of Committees and Sub-Committees. There is further another task on which a great deal of gratuitous and conscientious labour of the highest kind is bestowed, I allude to the examination of papers with a view to advising the Committee of Papers as to their publication. The past year has shown no flagging in scientific activity, in relation to papers brought before us.

Since the last Anniversary three parts of the 'Philosophical Transactions,' comprising upwards of 1160 pages of letterpress and 95 plates, and eight numbers of the 'Proceedings,' containing 1299 pages, have been published.

The preparation of the manuscript for another decade, 1874 to 1883, of the Royal Society's catalogue of scientific papers, is now almost complete. This great work has been extremely useful to men of science in enabling them at once to find where a memoir on a particular subject, written by an author whose name they know, as is usually the case, is to be found. To some extent it enables them also to find what has been written on a particular subject, for there are usually one or two authors, whose names they know, who have made it a special study, and on consulting their papers references are frequently found to the writings of others who have written on the same subject. Nevertheless it must be confessed that the value of the catalogue would be greatly increased if it could be accompanied by a key, of the nature of an *index rerum*. It was originally contemplated that this should be added, but the magnitude of the undertaking has hitherto prevented the Committee from attempting it. To be well done it would require the long-continued labour of a scientific staff representing different branches of science, and they could not be expected to engage in so heavy a work without adequate remuneration.

A great deal of work has been done during the past year in relation to the Library. More than 5,000 volumes have been removed to other rooms to make space for the more important works constantly accruing. A list of duplicates and deficiencies has been printed, and circulated among corresponding Societies. A shelf-catalogue is in progress, and is about a third of the way towards completion. Some work has also been done upon a catalogue of miscellaneous literature.

The electric lighting of the Society's apartments, which is now complete, seems to have given general satisfaction.

On the 31st of August this year, our distinguished Foreign Member,

M. Chevreul, attained his hundredth year. Rarely indeed is it given to anyone to see right through a century, more rarely still to retain his powers to such an age, yet both, I am happy to say, have been granted to M. Chevreul. In anticipation of this event, preparations were made for its due celebration. I received an invitation for our Fellows to assist at the celebration; but unfortunately it was at a time of year when most of us were scattered, and moreover time did not permit of making it generally known. I am afraid we had no representative at the actual ceremonial, but I am sure that none the less our hearts were with the veteran savant.

This year has also witnessed the celebration of the 250th anniversary of the University of Heidelberg. The Council had appointed our Foreign Secretary as a deputation to represent the Society on the occasion. Unfortunately when the time was close at hand, Dr. Williamson was prevented by the condition of his health from taking part in the celebration; but acting on the emergency on behalf of the Society, I requested our Fellow, Sir Henry Roscoe, to take his place, which he was so good as to do.

In his Presidential Address last year Professor Huxley suggested the idea, I may say expressed the hope, that the Royal Society might associate itself in some special way with all English-speaking men of science; that it might recognise their work in other ways than those afforded by the rare opportunities of election to our foreign membership, or the award of those medals which are open to persons of all nationalities alike. This suggestion has been taken up in one of our Colonies. We have received a letter from the Royal Society of Victoria, referring to this passage in the Address, and expressing a hope that in some way means might be found for establishing some kind of connexion between our own oldest scientific Society and those of the Colonies.

The Council have appointed a Committee to take this letter into consideration, and try if they could devise some suitable plan for carrying out the object sought. The Committee endeavoured at first to frame a scheme which should not be confined to the Colonies and Dependencies of the British Empire, but should embrace all English-speaking communities. But closely connected as we are with the United States by blood and language, they are of course politically a foreign nation, and this fact threw difficulties in the way of framing at once a more extended scheme, so that the Committee confined themselves to the Colonies and Dependencies of our own country, leaving the wider object for some future endeavour, should the country concerned seem to desire it. The scheme suggested was laid before the members of the present Council, but there was not an adequate opportunity of discussing it, and it will of course come before the new Council. Should they approve of some such measures

as those recommended by the Committee, they will doubtless assure themselves in some way or other that those measures are in accordance with the wishes of the Fellows at large before they are incorporated into the Statutes.

But in connexion with this subject there is another suggestion which I would venture to offer, and which I know has been thought of by others.

A good many years ago it was not unusual to elect to the Fellowship men of distinguished eminence in departments other than scientific. More recently a change was made in the Statutes whereby Privy Councillors are enabled to become Fellows by a special method, without interfering with the selection by the Council of fifteen from among the candidates, whom they recommend to the Society for election. This to a certain extent superseded the necessity of appealing to other than scientific claims, and in some respects the method had special advantages. Those who attained to a place on Her Majesty's Privy Council were sure to be distinguished men, whom we should be glad to welcome among us; and by confining the privilege of special election to these, with whose appointment the Council had nothing to do, all invidious distinctions were prevented. But the method has the disadvantage that it applies only to a particular class of merit. A man, for instance, might be of quite first rate eminence in poetry or literature, but that would not lead to a seat on the Privy Council. Such a man could only be elected by being placed on the selected list of fifteen. But it seems to me that there is something not quite courteous either to the eminent man himself, or to the scientific man who would have to be passed over to make room for him, in thus putting him in competition with those who seek admission on purely scientific grounds. I cannot help thinking that it might be well if the Council had the power of recommending for special election men of high distinction on other than scientific grounds, whose connexion with us would on both sides be felt to be an honour, and who, though not it may be themselves scientific, might usefully assist us by their counsel. I do not think it would be difficult to devise means for providing that such a privilege should be accorded only in case of very high eminence.

The application of photography to the delineation of celestial objects has of late years made rapid strides; and, partly owing to the improved sensitiveness of the plates, partly to greater exactness in regulating the motions of equatorially mounted telescopes, it has been found possible to photograph even minute stars. The question is accordingly now seriously entertained whether we may not trust to photography for the formation of star maps and star catalogues, taking eye observations on a sufficient number of stars here and there for reference, and trusting to differential measurements taken on the

plates for determining the positions of the other stars. Indeed I think the practicability of this application may now be considered as established, and there only remains the question of the best mode of carrying it out on a uniform plan. In the course of the autumn I had a letter from Admiral Mouchez, Director of the Paris Observatory, in which he informed me that in response to the presentation of specimens of the admirable star photographs taken by the MM. Henry, several of the astronomers to whom they had been sent suggested that it would be well that a conference of astronomers of various nations should be held, with a view to taking concerted action for obtaining on a uniform plan a complete map of the whole starry heavens. He wished accordingly to obtain an expression of opinion on the part of the Royal Society as to the desirableness of holding such a conference; and as it was contemplated, in case the proposal should be favourably entertained by those consulted, that the conference should be held at Paris in the spring, and it would be necessary to give timely notice to the astronomers who live in the southern hemisphere, an early reply was requested.

As it would have defeated Admiral Mouchez's object to wait till the Council should reassemble after the recess, I wrote at once to consult four of our Fellows specially named by Admiral Mouchez; and on receiving their replies I wrote to Admiral Mouchez, saying that under the circumstances I took it upon me to express in the name of the Royal Society our approval of the suggestion, explaining at the same time that I did so on the understanding, which I fully believed to be in accordance with his intention, that the astronomers who might attend the conference should not be considered as pledged to the adoption of the methods or scale of the MM. Henry, but that the whole subject should be open to discussion. On reporting what I had done to the Council when they met after the recess, I obtained an expression of their approval.

In these photographs a remarkable instance was exhibited of the power of photography to reveal the existence of objects wholly invisible to the eye. One of the stars of the Pleiades was found to be surrounded by a nebula which cannot be seen with telescopes. The reason of the difference of power of the plate and eye is very obvious; with the eye an object is either seen or not seen at once, whereas with the plate, provided there be an absence of stray light, feebleness of intensity can be made up for by length of exposure.

But the MM. Henry are by no means the only persons who have applied photography to the delineation of the stars. Among others our Fellow, Dr. Gill, who has sent us some excellent specimens of the photographs obtained by his instrument, proposes to take at the Cape Observatory photographs of the whole starry heavens of the southern hemisphere, under such conditions as to include the magnitudes

contained in Argelander's "Durchmusterung" of the northern hemisphere, and to subsequently reduce the observations so as to complete Argelander's great work by extending it to the southern hemisphere. Professor Kapteyn, in Holland, has nobly undertaken to devote his spare time for seven years to superintending the reduction. Dr. Gill has laid the proposal before the Government Grant Committee. Having regard to the magnitude of the undertaking, and the probability of a conference of astronomers being shortly held in Paris to discuss the whole question, the Government Grant Committee suggested to the Council of the Royal Society that they should appoint a committee to take the subject into consideration, and the Council have acted on this suggestion. Dr. Gill intends to come to Europe in the spring, so that the committee will be able to consult him personally.

This morning I received through the Foreign Office an invitation from the Académie des Sciences for myself, or some other Delegate of the Royal Society, to attend the conference to which I have already referred, which is fixed for the 16th of April. I shall take the first opportunity of consulting the new Council as to their wishes.

The Copley Medal for this year has been awarded to the veteran in science, our foreign member, Professor Franz Ernst Neumann, for his researches in theoretical optics and electro-dynamics.

Having in his earlier years treated of crystallographic subjects, he more than half-a-century ago turned his attention to the theory of light. Fresnel had, with his wonderful sagacity, arrived at his celebrated laws of double refraction from the theory of transverse vibrations, aided by conceptions derived from a dynamical theory which was only in part rigorous. Cauchy and Neumann, independently of each other, were the first to deduce from a rigorous dynamical calculation, applied to a particular hypothesis as to the constitution of the ether, laws of double refraction, not indeed absolutely identical with those of Fresnel, but closely resembling them. In this case the laws were known beforehand. But in a very elaborate later paper, Professor Neumann deduced from theory the laws of crystalline reflection, laws which appear to agree with the observations of Seebeck, and which had not been discovered by Fresnel, though some of them were independently and about simultaneously obtained by MacCullagh.

Professor Neumann is perhaps still better known in connexion with the theory of electrodynamics, and the mathematical deduction of the laws of induced currents due to the motion of the primary and secondary conductor. He may be said to have completed for the induction of currents the mathematical treatment which Ampère had applied to their mechanical action.

Of the two Royal Medals, it is the usual, though not invariable,

practice to award one for the mathematical and physical, and the other for the biological sciences.

One of these medals has this year been awarded to Professor Peter Guthrie Tait, for his various mathematical and physical researches.

Professor Tait is well known for his numerous and important papers in both pure mathematics and physics. The late Sir William Hamilton regarded him as his own successor in carrying on and completing the newly invented calculus of quaternions, of which Professor Tait is continually making new applications. Among his investigations in the domain of experimental physics may be mentioned his determination of the conducting powers of metals for heat by a method which appears to possess special advantages, and his investigation of the effect of extremely great pressures on thermometers, undertaken with a view to deducing correct results for the temperatures at great depths in the ocean from the observations made in the "Challenger" expedition. This latter subject has led him to investigate the behaviour, as to compressibility and development of heat, of liquids and solids under enormous pressures, a subject in which he is still engaged. Before concluding I must mention his elaborate papers on systems of knots, recently printed in the Transactions of the Royal Society of Edinburgh.

The other Royal Medal has been awarded to our Fellow, Mr. Francis Galton, for his statistical inquiries into biological phenomena.

Mr. Galton is well known as an explorer and geographer, and his mind is singularly fertile in the devising of ingenious instruments for various objects. Many years ago he brought before us some remarkable experiments instituted with a view to test a particular biological theory, in which rabbits of a pure variety were so connected with others of a different variety that the same blood circulated through both individuals, and the point to determine was whether this blood-relationship, in the most literal sense of the term, had any effect on the offspring. Contrary to what the theory in question led us to regard as the more probable, the result proved to be negative. It is, however, in accordance with the rules for the award of the Royal Medals, more especially the later investigations of Mr. Galton, in relation to vital statistics, that have been taken as the ground of the award. He has shown that by taking the average of a number of individuals having some condition in common, individual peculiarities apart from that condition are eliminated in the mean, and results are obtained which may be regarded as typical of that condition. One way of doing this is by his method of compound photographs. Thus we may obtain typical features of criminals of a particular kind, of consumptive persons, and so forth. By adhering to the method of averages, he has even succeeded in obtaining a mathematical expression, very closely verified by observation, con-

necting the mean deviation of some condition (such for example as stature) in a series of individuals from the general average with the mean deviations of the same condition in the relatives of those same individuals of different kinds, such as fathers, brothers, &c. Nor is the statistical method applicable to bodily characteristics alone. Mr. Galton has even extended it with remarkable ingenuity and originality to mental phenomena also.

The Rumford Medal has been awarded to Professor Samuel P. Langley, for his researches on the spectrum by means of the bolometer.

A better knowledge of the ultra-red region of the spectrum, which includes the larger part of the energy of solar radiation, had long been a desideratum, when Professor Langley commenced his work upon this subject. Finding the thermopile insufficiently sensitive for his purpose, he contrived the "bolometer." This instrument depends upon the effect of temperature upon the electrical resistance of metals, a quantity susceptible of very accurate measurement; and, with its aid, Professor Langley has been able to explore a part of the spectrum previously almost inaccessible to observation.

A result of Professor Langley's work, very important from the point of view of optical theory and of the ultimate constitution of matter, relates to the law of dispersion, or the dependence of refrangibility on wave-length. Cauchy's formula, which corresponds well with observation over most of the visible spectrum, is found to break down entirely when applied to the extreme ultra-red.

Professor Langley has given much attention to the important question of the influence of the atmosphere on solar radiation. The expedition to Mount Whitney, successfully conducted by him in face of many difficulties, has given results of the utmost value, pointing to conclusions of great interest and novelty.

The Davy Medal has been awarded to our foreign member, M. Jean Charles Galissard de Marignac, for his researches on atomic weights.

M. Marignac's numerous researches on atomic weights, which have been continued for a great number of years, have played an exceedingly important part in establishing and consolidating that groundwork of chemistry. They are remarkable for originality in devising methods appropriate to the respective cases, the most conscientious care in discovering errors which occurred in the respective operations, and indefatigable perseverance in finding means to eliminate the disturbing influences. His labours are all the more valuable because he chose for their field chiefly those elements which are most generally used in chemistry, and are most important to chemists, and on which the determination of new atomic weights is most generally made to depend.

The Statutes relating to the election of Council and Officers were then read, and Sir Erasmus Ommaney and Mr. Stainton having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Professor George Gabriel Stokes, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.—Professor Michael Foster, M.A., M.D.; The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Professor Alexander William Williamson, LL.D.

Other Members of the Council.

Professor Robert B. Clifton, M.A.; Professor George Howard Darwin, M.A., LL.D.; W. T. Thiselton Dyer, M.A.; Professor David Ferrier, M.A.; Edward Frankland, D.C.L.; Arthur Gamgee, M.D.; Archibald Geikie, LL.D.; Professor Joseph Henry Gilbert, M.A.; John Hopkinson, M.A., D.Sc.; J. Norman Lockyer, F.R.A.S.; Sir Lyon Playfair, K.C.B., LL.D.; Professor Bartholomew Price, M.A.; Professor Pritchard, M.A.; Admiral Sir George Henry Richards, K.C.B.; Professor Arthur Schuster, Ph.D.; Philip Lutley Sclater, M.A.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1885 ..	5	45	196	182	87	515
Since Elected ..		+ 4	+ 3	+ 1	+ 13	+ 21
Since Withdrawn				− 1		− 1
Since Deceased ..			− 7	− 10		− 17
Nov. 30, 1886 ..	5	49	192	172	100	518

Balance Sheet, 1886.

Statement of Receipts and Expenditure from November 14, 1885, to November 13, 1886.

	£	s.	d.	£	s.	d.
To Balance at bank, 14th November, 1885	£1,183	1	3			
Less Trust Funds (see below)	717	12	8	465	8	7
,, Balance in hand, Catalogue Account	17	9	7			
,, ,, Petty Cash	14	17	5	32	7	0
,, Annual Contributions, 175 at £4	700	0	0			
,, 100 at £3	300	0	0	1,000	0	0
,, Admission Fees				20	0	0
,, Compositions				180	0	0
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions				250	0	0
,, Rents:	£	s.	d.			
Fee Farm, Lewes	18	11	2			
Mablethorpe Estate	92	16	11	111	8	1
,, Ground Rents				673	0	10
,, Dividends (exclusive of Trust Funds)				2,525	7	2
do. Jodrell Fund				150	5	10
,, Interest on Mortgage Loan				580	0	0
,, Sale of Transactions and Proceedings				1,218	6	5
,, Balance of Naturalists' Account				20	4	10
				£7,226	8	9

	£	s.	d.	£	s.	d.
By Salaries, Wages, and Pension				1,469	16	0
,, Catalogue of Scientific Papers				187	9	0
,, Books for the Library				254	1	5
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	368	12	7			
,, Ditto Proceedings, Nos. 239 to 245	735	15	3			
,, Ditto Miscellaneous	62	2	6			
,, Paper for Transactions and Proceedings	418	7	10	2,672	3	11
,, Binding ditto	50	0	0			
,, Engraving and Lithography	1,037	5	9			
,, Soirée and Reception Expenses				97	11	11
,, Coal, Lighting, &c.	101	15	5			
,, Office Expenses	55	10	0½			
,, House Expenses	125	2	4½			
,, Tea Expenses	20	16	11½			
,, Fire Insurance	56	15	0			
,, Taxes	61	15	3	566	3	0½
,, Advertising	18	19	0			
,, Postage (including Scientific Relief Circulars), Parcels, and Petty Charges	94	11	3			
,, Miscellaneous Expenses	30	17	9			
,, Law Charges				37	19	10
,, Eclipse Expedition, Cash advanced (making with previous advances £131 3*s.* 1*d.* due)				78	7	10
,, Balance on hand, Catalogue Account	13	9	7			
,, Ditto, Petty Cash	11	0	4½	24	9	11½
,, Balance at Bankers'				1,838	5	10
				£7,226	8	9

Trust Funds.

To Balance at Bank 14th November, 1885 :—	£	s.	d.	£	s.	d.
General Account	717	12	8			
Darwin Memorial Fund Account	368	5	2			
Gassiot Trust Account	167	13	5	2,511	10	8
Circumpolar Expedition Account	587	11	5			
Fee Reduction Fund Account	576	10	6			
Scientific Relief Fund Account	93	17	6			
„ Donation Fund, Dividends and Transfer from Handley Fund	413	4	6			
„ Rumford Fund, Dividends	67	7	3			
„ Wintringham Fund „	34	15	10			
„ Bakerian and Copley Medal Fund, Dividends	58	1	6			
„ Davy Medal Fund, Dividends	35	8	7			
„ Croonian Lecture Fund, Rent received	46	13	8			
„ Darwin Memorial Fund, Dividends	77	6	8			
„ Gassiot Trust, Dividends	495	13	7	7,595	16	5
„ Keck Bequest do.	23	4	0			
„ Circumpolar Expedition	0	0	0			
„ Fee Reduction Fund, Dividends and proceeds of Whitworth Land Company Shares	2,553	13	2			
„ Scientific Relief Fund, Dividends, Donations, and Bentham Bequest	3,640	1	10			
„ Jodrell Fund, Dividends	150	5	10			
				£10,107	7	1

By Donation Fund, Grants, &c.	£	s.	d.	£	s.	d.
	422	4	8			
„ Rumford Fund						
„ Wintringham Fund, Payment to Foundling Hospital	34	17	6			
„ Bakerian and Copley Medal Fund, Lectures, Medal, and Gift	64	18	1			
„ Davy Medal Fund, Gold Medals	32	6	0			
„ Croonian Lecture Fund, Payments	52	8	11			
„ Darwin Memorial Fund, Printing	7	9	6			
„ Gassiot Trust, Payments to Kew Committee	489	17	7	6,911	16	6
„ Keck Bequest, Payment to Foreign Secretary	23	4	0			
„ Circumpolar Expedition, Expenses	167	4	8			
„ Fee Reduction Fund, Investments, &c.	3,098	10	10			
„ Scientific Relief Fund, Investments and Grants	2,368	8	11			
„ Jodrell Fund, Transfer to Royal Society General Account	150	5	10			
„ Balance at Bankers' :—						
General Account	1,378	0	7			
Circumpolar Expedition Fund	420	6	9			
Fee Reduction Fund	31	12	10	3,195	10	7
Scientific Relief Fund	1,365	10	5			
				£10,107	7	1

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £110 per annum.

Ground Rent of House No. 57, Basinghall Street, rent £380 per annum.

„ „ of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s.* per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Government Annuities and Bank Stock (produced £534 19*s.* 9*d.* in 1885–86).

£21,000 Reduced 3 per Cent. Annuities { £14,952 12*s.* 3*d.*, General Purposes.
£6,047 7*s.* 9*d.*, Handley Fund.

£15,000 Mortgage Loan, 4 per Cent.

£19,314 0*s.* 2*d.* Consolidated Bank Annuities, { being £15,861 19*s.* 1*d.*, namely :—

	£	*s.*	*d.*
Donation Fund	6,339	0	1
Rumford Fund	2,322	19	0
Wintringham Fund	1,200	0	0
Gassiot Trust	200	0	0
Sir J. Copley Fund	1,666	13	4
General Purposes	4,133	6	8

and £3,452 1*s.* 1*d.* in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s.* 8*d.* New 2½ per Cent. Stock.—Bakerian and Copley Medal Fund.

£12,182 14*s.* 10*d.* New Threes { £7,000, Scientific Relief Fund.
5,182 14*s.* 10*d.*, Jodrell Fund.

Estates and Property of the Royal Society—continued.

£667 5*s.* 6*d.* India Fours.

£600 Midland 4 °/₀, Debenture Stock.—Keek Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. / Davy Medal Fund, £660.

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£3,796 Great Northern Railway 4 per Cent. Debentures { Scientific Relief Fund, £2,400. / The Trevelyan Bequest, £1,396.

£4,000 Metropolitan 3½ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£5,000 North Eastern Railway 4 °/₀ Preference Stock.—General Purposes.

£5,000 London and North Western Consolidated 4°/₀ Preference Stock.—General Purposes.

£2,000 South Eastern Railway 4°/₀ Debenture Stock.—Darwin Memorial Fund.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

G. G. STOKES.
BARTHOLOMEW PRICE.
ARCHIBALD GEIKIE.
W. J. RUSSELL.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

T. G. BONNEY.
JOHN RAE.
JAMES COCKLE.
H. T. STAINTON.

Trust Funds. 1886.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	7,000	0	0
Great Northern 4 per cent. Debenture Stock	2,400	0	0
Uninvested	1,332	5	0
	£10,732	5	0

Dr. *Cr.*

	£	s.	d.	£	s.	d.
To Balance { Uninvested	33	11	3			
To Balance { In hand	60	6	3			
				93	17	6
„ Dividends				271	19	2
„ Bentham Bequest				67	11	2
„ Donations				3,300	11	6
				£3,733	19	4

	£	s.	d.	£	s.	d.
By Grants				299	0	0
„ Purchase of £1,000 Great Northern Railway 4 per cent. Debenture Stock				1,222	9	0
„ Purchase of £844 17*s.* 7*d.* New 3 per Cents.				846	19	11
„ Balance uninvested	£1,332	5	0			
„ „ in hand	33	5	5	1,365	10	5
				£3,733	19	4

Donation Fund.

£6,339 0*s.* 1*d.* Consols.
The Trevelyan Bequest. £1,396 Great Northern Railway 4 per Cent. Debenture Stock.

	£	s.	d.	£	s.	d.
To Balance				426	16	5
„ Dividends				237	17	0
„ Transferred from Handley Fund				175	7	6
				£840	0	11

	£	s.	d.	£	s.	d.
By Grants				414	10	0
„ Dividend credited in error last year				7	14	8
„ Balance				417	16	3
				£840	0	11

Rumford Fund.

£2,322 19*s.* Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	67	10	4	By Balance	134	17	7
„ Dividends, 1886	67	7	3				
	£134	17	7		£134	17	7

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* Consols.
£403 9*s.* 8*d.* New 2½ per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	108	7	1	By Gold Medal	4	10	7
„ Dividends, New 2½ per Cent. Stock	9	15	0	„ Prof. Kekulé, Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	48	6	6	„ Lectures, 1885 and 1886	10	7	6
				„ Balance	101	10	6
	£166	8	7		£166	8	7

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1886	23	4	0	By Payment to Foreign Secretary	23	4	0

Wintringham Fund.

£1,200 Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	34	17	6	By Payment to Foundling Hospital, 1886	34	17	6
„ Dividends, 1886	34	15	10	„ Balance	34	15	10
	£69	1	4		£69	13	4

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Balance	5	15	3	By Poor of St. James's, per Rev. J. E. Kempe	2	18	9
„ Rent	46	13	8	„ Law Expenses	9	17	0
				„ Dr. L. C. Wooldridge, Lecture	39	13	2
	£52	8	11		£52	8	11

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	74	6	1	By Gold Medals	32	6	0
„ Dividends	35	8	7	„ Balance	77	8	8
	£109	14	8		£109	14	8

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£200 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	167	13	5	By Payments to Kew Committee	489	17	7
„ Dividends	495	13	7	„ Balance	173	9	5
	£663	7	0		£663	7	0

Handley Fund.

£6,047 7*s.* 9*d* Reduced 3 per Cent. Annuities.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1886	175	7	6	By transferred to Donation Fund	175	7	6

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1886	150	5	10	By transferred to Royal Society General Account	150	5	10

Fee Reduction Fund.

£4,000 Metropolitan Consols 3½ per Cent.
£7,000 London and North Western Railway 4 per Cent. Debentures.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	576	10	6	By transferred to Royal Society General Account (1886)	250	0	0
„ Dividends, 1886	360	5	6	„ £1,800 Metropolitan 3½ per Cent. Stock purchased	1,972	10	0
„ Ditto on London and North-Western Railway Preference Stock, incorrectly paid to this account this year, transferred to General Account per contra	193	7	8	„ Transferred to General Account Nine Dividends on London and North-Western Railway Preference Stock, incorrectly brought into this account from 1882 to 1886	876	0	10
„ Proceeds of 200 Shares Whitworth Land Company, Limited	2,000	0	0	„ Balance	31	12	10
	£3,130	3	8		£3,130	3	8

Darwin Memorial Fund.

£2,000 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	368	5	2	By Printing	7	9	6
„ Dividends, 1886	77	6	8	„ Balance	438	2	4
	£445	11	10		£445	11	10

ADMINISTRATIVE FUND.

Circumpolar Expedition Fund.

	£	s.	d.		£	s.	d.
To Balance	587	11	5	By Expenses	167	4	8
				„ Balance	420	6	9
	£587	11	5		£587	11	5

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XXXIX, p. 310).

1885–86.

	£
Dr. D. Gill, for continuing the daily photographs of the Corona, and Star-charts of the Southern Hemisphere by direct photography	300
H. B. Dixon, for apparatus and assistance in experimenting on the Explosion of Gases	100
H. Tomlinson, for apparatus used in an investigation of the Influence of Stress and Strain on the Physical Properties of Matter	50
Prof. W. N. Hartley, for assistance and appliances to be used in continuing Researches on the Application of Spectrum-photography to Quantitative Chemical Analysis	100
Prof. C. Lapworth, for aiding in collecting and studying the Graptolites of Wales and the West of England	50
Dr. Wooldridge, for a research on the Physiology and Pathology of Blood	35
F. Gotch, for investigation of the physiological Properties and anatomical Structure of the electrical organ of *Malapterurus electricus*	75
A Committee of the Royal Irish Academy, for Dredging operations off the S.W. Coast of Ireland	100
Dr. W. R. Dunstan, for investigation of the physiological Properties of the glucoside Loganin which occurs in *Strychnos nux-vomica*	75
Dr. F. R. Japp, for an investigation of the reactions of Ketones, Diketones, and allied compounds	75
Prof. Humpidge, for apparatus to continue a Research on the Specific Heats of the Solid Elements in the pure state and at varying temperatures	50
Prof. Humpidge, for his Research on Metallic Glucinum	12
W. E. Adeney, for completion of the work of determining the Wave-lengths and Mapping the Lines of the Ultra-violet Spark-spectra of Nickel, Cobalt, Palladium, Gold, and Platinum	50
The Scottish Meteorological Society, for an investigation on Sea-temperature, and a discussion of observations from 1855 to present time	50
Carried forward	£1,121

Brought forward	£1,122
W. Doberck, for aid in making sufficient Observations to construct Hygrometric Tables for use with Rotating Thermometers	100
W. W. Haldane Gee, for determining certain Magnetic Constants of Iron, Nickel, and Cobalt, and their alloys; especially to investigate the temperature coefficients of magnets made from the materials	25
Dr. D. Gill, for constructing an improved apparatus for Photographing Star Maps	100
W. F. Denning, for observation of Shooting Stars, and of their radiant points, with special reference to the long duration and stationary position of many meteor showers	40
G. J. Symons, for studying the Barometric Oscillations of short period which occur only during thunderstorms	22
G. J. Symons, for renewing Rain-gauges on certain mountains in the English Lake District, and in Wales	30
J. H. Poynting, for the determination of the Mean Density of the Earth by the ordinary balance	40
The Eclipse Committee of the Royal Society, towards the expenses of the Eclipse (1886) Expedition	400
The Pendulum Committee of the Royal Society, for the expense of Pendulum Observations to be made at the Kew Observatory	150
G. R. Vine, for further work upon and descriptions of British Fossil Polyzoa and other organisms	20
S. J. Hickson, for the hire of boats and labour in the pursuit of work upon the Anatomy and Development of Millepora, &c.	100
H. T. Stainton, in aid of publication fund of the Zoological Record Association	100
J. Starkie Gardner, for concluding investigations into the Fossil Floras of the Basaltic Formations of Ireland and Scotland	50
The Liverpool Marine Biology Committee, for the exploration of the Sea Bottom by Dredgings in Liverpool Bay and the neighbourhood	25
W. C. McIntosh, for continuation of Researches on the ova and development of the Food and other Fishes, and on the reproduction of the Common Mussel	100
Percy F. Kendall, for the exploration by excavation and the removal to Manchester of a large quantity of the Clay of the recently discovered Pliocene deposit of St. Erth, Cornwall	30
Carried forward	£2,454

Brought forward	£2,454
W. Bateson, for an investigation of the Fauna of the fresh-water and salt Lakes of the Steppes lying to the North of the Sea of Aral	200
Rev. A. E. Eaton, for cost of illustrations of Terrestrial Isopod Crustacea	100
J. T. Cunningham, for continuing a Research on the Development of Marine Teleostean Fishes, and of *Myxine glutinosa*	100
Dr. P. F. Frankland, for apparatus and chemicals to be used in his investigations on the Micro-organisms in Air and Water	20
Spencer Pickering, for further investigations on Molecular Compounds	100
Prof. W. K. Parker, for continuation of Researches into the Morphology of the Vertebrata	300
Prof. T. Rupert Jones, for examination and description of the Fossil Entomostraca (Ostracoda)	100
	£3,374

Dr.

	£	*s.*	*d.*
To Grant from Treasury	4,000	0	0
To Repayment	47	6	3
To Interest on Deposit	3	12	0
	£4,050	18	3

Cr.

	£	*s.*	*d.*
By Balance overdrawn, Nov. 30, 1885	301	12	5
By Appropriations, as above	3,374	0	0
Printing, Postage, Advertising, and other Administrative Expenses	95	10	6
By Balance on hand, Nov. 30, 1886	279	15	4
	£4,050	18	3

Account of Grants from the Donation Fund in 1885–86.

	£	*s.*	*d.*
Mr. W. W. Watts, for a research on the Petrology of the Volcanic Rocks of Corndon	15	0	0
Mr. Norman Collie, for a research on the action of Hydrochloric Acid, and Haloid compounds of Hydrocarbon radicles, on the Amide of Acetoacetic Ethyl Ether	15	0	0
Dr. Cash, for an inquiry on Intestinal Movements	15	0	0
Dr. Gaskell, to assist in his researches on the relation of the cranial to the spinal Nerves, and on the anatomy of the visceral Nerves	30	0	0
Prof. Schäfer and Mr. J. R. Bradford, for a research upon the electrical conditions of secreting Glands	25	0	0
Rev. S. J. Perry and Prof. Balfour Stewart, to assist in their comparison of the Magnetograph Curves of Kew and Stonyhurst	30	0	0
Captain Abney, for the expenses of the Bakerian Lecture	2	0	0
Mr. Crookes, for aid in his researches on Yttria	50	0	0
Prof. Rupert Jones, for illustrations of the Fossil Entomostraca (Ostracoda)	25	0	0
Dr. McMunn, for purchase of a Microspectroscope with a view to further examination of the Spectra of the organs and tissues of animals	12	10	0
Dr. Cash, for a pharmacological investigation into the action of some of the Aromatic Compounds which have not as yet been examined	20	0	0
Mr. G. J. Symons, towards the cost of Thermometric Observations on the tower of Lincoln Minster	20	0	0
Mr. Sherrington, for aid to pursue in Italy the researches on Cholera, in which he was engaged in Spain in conjunction with Prof. Roy	100	0	0
Mr. Warren de la Rue, for the completing of his Catalogue of Latitudes and Longitudes of Solar Spots, £200. On account.	55	0	0
	£414	10	0

Report of the Kew Committee for the Year ending October 31, 1886.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

Mr. Warren de la Rue, *Chairman.*

Captain W. de W. Abney, R.E.	The Earl of Rosse.
Prof. W. G. Adams.	Mr. R. H. Scott.
Prof. G. C. Foster.	Lieut.-General W. J. Smythe.
Mr. F. Galton.	Lieut.-Gen. R. Strachey, C.S.I.
Admiral Sir G. H. Richards, K.C.B.	Lieut.-General J. T. Walker, C.B.

The Committee regret to announce the death, in December last, of one of their members, Captain Sir F. Evans, K.C.B., formerly Hydrographer. He had held a seat upon the Committee since 1874, and was a frequent attendant at their meetings, rendering most valuable assistance in all questions relating to terrestrial magnetism or navigation brought forward for consideration.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have been in constant operation during the year, and in accordance with the usual practice, determinations of the scale values of all the instruments were made early in January.

The Vertical Force Balance Magnet was found to have a scale value of for 1 inch $\delta V = 0{\cdot}0296$, and therefore appeared wanting in sensitiveness, it was accordingly re-adjusted and brought up to the proper pitch of delicacy.

The values of the ordinates of the different photographic curves determined then were as follows :—

Declination : 1 inch = 0° 22′·04. 1 cm. = 0° 8′·7.

Bifilar, January 11, 1886, for 1 inch $\delta H = 0{\cdot}0268$ foot grain unit.
„ 1 cm. „ $= 0{\cdot}0005$ C.G.S. unit.
Balance, January 19, 1886 „ 1 inch $\delta V = 0{\cdot}0274$ foot grain unit.
„ 1 cm. „ $= 0{\cdot}0005$ C.G.S. unit.

The chief days on which notable magnetic disturbance was recorded were as follows :—January 9, March 30, July 27, and October 7–11.

The magnetic instruments have been studied, and a knowledge of their manipulation obtained by Professor L. M. Russell, Mr. E. Kitto, and Mr. C. Chambers, jun.

Professors Rücker and Thorpe visited the Observatory in April, and made several sets of observations with the instruments which they have employed in their magnetic survey of the British Isles, prior to their commencing operations on the southern section, which have occupied them during the past summer.

At the request of the Royal Cornwall Polytechnic Society, a set of magnetographs on an improved model has been constructed for the Committee by Mr. Munro, which, after a lengthened trial in the Verification House, were forwarded to Falmouth, and erected at the New Observatory, under the supervision of Mr. T. W. Baker. The cost has been defrayed by a grant from the Royal Society's Government Fund.

At the suggestion of General Sir J. H. Lefroy, the Committee have caused a plate to be engraved on which sectional lines are laid down on the scale adopted by the International Polar Conference, for plotting all magnetic curves on a uniform system. Impressions from this plate will be kept at the Observatory, and supplied at cost price to persons desirous of making use of such forms.

Information on matters relating to terrestrial magnetism and various data, have been supplied to Professor W. G. Adams, Dr. Atkinson, General Sir J. H. Lefroy, Professor B. Stewart, M. Moureau, Captain Schück, and others.

The monthly observations with the absolute instruments have been made as usual, and the results are given in the tables forming Appendix I of this Report.

The following is a summary of the number of magnetic observations made during the year :—

Determinations of	Horizontal Intensity	30
,,	Inclination	155
,,	Absolute Declination	36

The diurnal range of the Declination having become a somewhat interesting feature in magnetic reductions, an additional table, giving the values for the summer and winter seasons and for the whole year, as determined from selected curves by the graphic method,* has been inserted in the Appendix.

II. Meteorological Observations.

The several self-recording instruments for the continuous registration respectively of atmospheric pressure, temperature, and humidity, wind (direction and velocity), bright sunshine, and rain, have been maintained in regular operation throughout the year.

The only alterations made in the above instruments have been the following: a screen of blue glass has been interposed in the barograph between the barometer tube and the light, with the result of improving the definition of the photographic curve, and a Stonyhurst lifter has been fitted to the Beckley rain-gauge, causing the pencil to return to its original position after depression more rapidly than it did previously.

The standard eye observations for the control of the automatic records have been duly registered during the year, together with the daily observations in connexion with the U.S. Signal Service synchronous system. A summary of these observations is given in Appendix II.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted to the Meteorological Office.

The terrestrial radiation thermometer (grass minimum) was found broken on July 11, and replaced by a new instrument on July 21.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of	standard barometer	1725
,,	dry and wet thermometers	3450
,,	maximum and minimum thermometers	730
,,	radiation thermometers	1480
,,	rain gauges	730
Cloud and weather observations		1877
Measurements of barograph curves		8751

* See paper by Mr. Whipple in the 'Quart. Jour. Roy. Met. Soc.,' vol. 9, p. 45.

Measurements of	dry bulb thermograph curves..	9473
,,	wet bulb thermograph curves..	8681
,,	wind (direction and velocity)..	17515
,,	rainfall curves	740
,,	sunshine traces.....	2113

In compliance with the usual request made by the Meteorological Council to the Committee, Mr. Whipple visited the Observatories at Aberdeen, Glasgow, and Stonyhurst, and the anemograph at Swaubister. He also superintended the erection of new instruments at North Shields and Fleetwood.

Mr. Baker has visited the Valencia and Falmouth Observatories for the purpose of inspection.

With the sanction of the Meteorological Council, weekly abstracts of the Meteorological results have been regularly forwarded to, and published by 'The Times' and 'The Torquay Directory.' Data have also been supplied to the Council of the Royal Meteorological Society, the editor of 'Symons's Monthly Meteorological Magazine,' the Secretary of the Institute of Mining Engineers, Messrs. B. Latham, Gwilliam, Rowland, and others. The cost of these abstracts is borne by the recipients.

Electrograph.—Acting upon the recommendation of the Kew Committee the Meteorological Council have purchased a new quadrant electrometer, constructed on Mr. de la Rue's principle, with Professor Clifton's improvements, together with a chloride of silver battery of 60 cells, for the purpose of maintaining the potential of the quadrants at a certain point.

By the kindness of the Chairman of the Committee, experiments were made at his laboratory in Portland Place by means of which the scale value of the instrument was determined before it was conveyed to the Observatory, and erected in the place of the Thomson instrument formerly employed.

No change has been made in the recording apparatus attached to it. The instrument has been working for the past month in a satisfactory manner.

In accordance with a request made by the Meteorological Council, and at their expense, the electrograms for the two years 1882 and 1883 have been tabulated in absolute values.

III. Solar Observations.

The sketches of Sun-spots, as seen projected on the photoheliograph screen, have been made on 169 days, in order to continue Schwabe's enumeration, the results being given in Appendix II, Table IV.

Transit Observations.—301 observations of solar and 76 of sidereal transits have been taken, for the purpose of keeping correct local time

at the Observatory, and the clocks and chronometers have also been compared daily. The Observatory Chronometers Arnold 86 and Parkinson and Frodsham 2408, have been cleaned and re-adjusted, and the mean-time clocks, Shelton K. O., and Shelton 35, examined and re-adjusted by Dent.

The following clocks, French, Dent 2011, Shelton K. O., and the chronometers, Molyneux No. 2125 and Breguet No. 3140, are kept carefully rated as time-keepers at the Observatory.

The mean-time clock, Shelton 35, after cleaning, &c., was bolted to the wall of the chronometer-room for use in daily comparisons with the chronometers on trial.

In order to facilitate the inter-comparison of the clocks, the chronometer "Parkinson" has been specially fitted up as a "hack" instrument.

At the request of the Council of the Royal Meteorological Society, certain experiments were made with the view of investigating Professor W. K. Zenger's solar phenomena and an examination was also made of the Kew solar photographs. The results obtained were however of a negative character only. A note of them has been published in the 'Quarterly Journal Roy. Met. Soc.,' vol. 12, p. 215.

A comparative trial extending over five months was made of Professor McLeod's sunshine recorder (see 'Phys. Soc. Proc.,' vol. 6, p. 216), and the Stokes' instrument, which proved the results given by the two instruments to be practically identical.

IV. Experimental Work.

Photo-nephograph.—The report on last year's work in cloud photography was duly submitted to the Meteorological Council, and placed by them in the hands of Professor Stokes for consideration.

Professor Stokes having investigated the methods employed at the Observatory, devised a new graphic process for determining the cloud heights and motions in a much simpler manner than by the use of mathematical formulæ only. He invented a special apparatus called a projector, which has been constructed by C. Baker, and is now being utilized in the reduction of the pictures taken during the past season. These have amounted to 112 cloud negatives, and were obtained in 15 days.

For convenience of dealing with the cloud pictures in the projection apparatus with greater facility, the negatives have all been printed off on paper prepared by the cyanotype process.

Certain minor additions were made to the cameras, and accessory apparatus, which have tended to facilitate their working, and their action has been fairly satisfactory ever since. There is, however, still an occasional failure due to uncertainty of the duration of the

time of exposure of the twin cameras, although this is apparently instantaneous.

Solar Radiation Thermometers.—The experiments with these thermometers have been continued during the past summer months, and at times as many as 8 instruments have been under observation.

It having appeared that during the winter the *vacua* in certain of the instruments had deteriorated either by leakage or evolution of gas from the lamp-black coating of the bulbs, experiments were made in the Chairman's laboratory which proved that such was the case. New thermometers were made and enclosed in jackets not provided with platinum electrodes; the bulbs were also made of black glass, having the stems covered with black enamel. These, after careful exhaustion, were placed under observation, but did not register temperatures higher than had been previously observed.

Advantage was taken of an offer kindly made by Professor Thorpe to make solar radiation observations on his recent Eclipse Expedition to Grenada, and two of the instruments were lent to him. He has now returned them to the Observatory, together with copies of the readings he was able to procure on the occasion.

Electrical Anemograph.—This instrument, after a lengthened trial in the Experimental House and the execution of certain minor alterations by Mr. Kempe, has been dismounted and forwarded to the Valencia Observatory by instructions of the Meteorological Council. The external parts were previously put into thorough repair by Mr. Munro, in order to fit the anemograph for the rough weather to which it will be exposed when erected on a hill almost overlooking the Atlantic.

Dines' Anemometer.—Trials have been made of two anemometers constructed on a new principle by Mr. W. H. Dines, B.A. Owing, however, to structural defects, both instruments broke down before any final results were obtained, and were returned to the maker for repair.

Glycerine Barometer.—This instrument, having very considerably deteriorated by age, was dismounted by Mr. Jordan in June, and after thorough cleaning and repair by Mr. J. Steward, was again erected and refilled with new fluid.

Pendulum Experiments.—At the request of General Walker, certain experiments were made with the view of ascertaining the stability of the Experimental House as a site for pendulum operations. These having proved that building unsuitable, a wooden erection 13 ft. × 9 ft. × 8 ft. has been constructed, at the desire of the Pendulum Committee of the Royal Society, in the lower South Hall of the Observatory, on the spot occupied in 1873 by Captain Heaviside, when experimenting with the Russian pendulum. (See Report for 1873.)

In this room it is the intention to erect the Indian Pendulum Appa-

ratus recently returned by Professor Peirce from the United States, and swing the pendulums so as to obtain a differential connexion with recent swings in New York. Subsequently, it is intended to convey the whole to the Royal Observatory at Greenwich, where other series of observations and experiments will be conducted with a view to connecting Greenwich with Kew.

At the request of the Meteorological Council, various experiments were tried with the view of selecting a suitable paper for use with the Beckley anemographs, and also of remedying certain defects found attendant on the employment of gelatinised photographic curves in processes of mechanical reduction in the Meteorological Office.

V. Verification of Instruments.

The following magnetic instruments have been verified, and their constants determined:—

1 Unifilar Magnetometer and an Inclinometer for the Falmouth Observatory.

Two Inclinometers have been purchased on commission for the Bureau of Navigation, Washington, 1 each also for the Ekaterinburg Observatory and the Lighthouse Board, Helsingfors. 2 Dip Needles have been procured and tested for the Mauritius Observatory; also 1 pair for the Lisbon Observatory; 7 Thomson's patent compasses with 7 vertical force instruments have also been examined for the Imperial Japanese Navy.

1 Unifilar, 2 Compasses, and 2 Inclinometers have been tested for opticians, and 2 Unifilars are at present undergoing verification.

The total number of other instruments compared in the past year was as follows:—

Barometers, Standard	31
,, Marine and Station	92
Aneroids	124
Total	247

Thermometers, ordinary Meteorological	1320
,, Standard and Chemical	210
,, Mountain	45
,, Clinical	9054
,, Avitreous	816
,, Solar radiation	45
Total	11490

Hydrometers	512
Anemometers	15
Rain Gauges	6
Sextants	139
Index and Horizon Glasses, unmounted	170
Dark Glasses, unmounted	597
Theodolites	9

Besides these, 32 Deep-sea Thermometers have been tested, 16 of which were subjected, in the hydraulic press, without injury, to pressures exceeding two tons on the square inch. 27 Thermometers have been compared at the freezing-point of mercury, making a total of 11549 for the year.

Duplicate copies of corrections have been supplied in 84 cases.

The number of instruments rejected on account of excessive error, or which from other causes did not record with sufficient accuracy, was as follows:—

Thermometers, clinical	67
„ ordinary meteorological	49
Various	112

7 Standard Thermometers have also been calibrated, and supplied to societies and individuals during the year.

1 Redier Barograph, 1 Richard Hygrometer, 1 Air Meter, 2 Telescopes, &c., were also examined.

There are at present in the Observatory undergoing verification, 43 Barometers, 904 Thermometers, 2 Hydrometers, and 27 Sextants.

A question having arisen as to the true interpretation of the tables of Specific Gravity used in connexion with the verification of instruments of the hydrometer class, a Sub-Committee has been appointed to consider the matter and authorize the adoption by the Observatory of certain definite rules.

VI. Rating of Watches.

The arrangements for rating watches mentioned in previous Reports have been continued during the year with great success, and up to the present 834 watches have been examined, of which 16 were submitted by the owners, and 818 by the manufacturers or dealers.

490 watches were received as contrasted with 302 received during the corresponding period of last year. They were entered for testing in the following classes:—

For class A, 436; class B, 36; and class C, 18. Of these 148 failed to gain any certificate; 16 passed in C, 102 in B, 224 in A, and 8 obtained the highest possible form of certificate, the class A *especially good.*

All the watches which obtained Class A certificates had marks assigned to them, indicating the degree of relative efficiency they exhibited during their trial, according to the following scale:—

The number of marks awarded to a watch that only just succeeds in obtaining an A certificate is 0, while that awarded to an *absolutely perfect watch* would be 100, made up as follows:—40 for a complete absence of variation of daily rate, 40 for absolute freedom from change of rate with change of position, and 20 for perfect compensation for effects of temperature.

In Appendix III will be found a statement giving the results of trial of the 20 watches which obtained the highest numbers of marks during the year, the first position being again attained—with 86·7 marks—by the maker who occupied it—with 86·1 marks—last year.

The number of watches obtaining a high figure in the marks list has, however, much increased.

The following table will indicate the nature of the trials to which ordinary certificates refer:—

Position of watch during test.	For certificate of Class		
	A.	B.	C.
Vertical, with pendant up	10 days	14 days	8 days
" " " right	5 "	—	—
" " " left	5 "	—	—
Horizontal, with dial up	5 "	14 days	8 days
" " " down	5 "	—	—
" at temp. 40° F.	5 "	1 day	—
" " 85° F.	5 "	1 "	—
Not rated	5 "	1 "	—
Total duration of test	45 days	31 days	16 days

Owing to the inconvenience and delay attendant on the employment of one safe for both hot and cold tests, a second was procured, and has been fitted up as a refrigerator, thereby enabling two sets of watch trials to proceed simultaneously, and more constant temperatures in both heat and cold to be sustained for the necessary periods.

Special attention has been given to the examination of *pocket chronographs*, in accordance with the request of the Cyclists' Union.

Manufacturers have also been advised of certain mechanical defects in the action of the chronograph work, and latterly an improvement has taken place in respect of this.

Early in the year a communication was received from the President

of the Section d'Horlogerie de la Société des Arts à Genève, asking for full particulars of the system of rating at this Observatory. These were forwarded to him, and in acknowledging receipt of the same he expressed the gratification of his Council at the degree of accuracy obtained in the Kew trials during the year.

Rating of Chronometers.—Application having been made to the Committee to extend their system of watch rating to marine chronometers, arrangements were carried out for effecting this. A chronometer oven formerly constructed for the Testing Office of the Board of Trade, being unemployed, was obtained on loan from the Meteorological Council, in whose possession it was, and erected with the necessary gas-fittings in the Thermograph-room of the Observatory.

A scheme for rating and certifying was drawn up, of which the following is a brief abstract.

The trial occupies 35 days, divided into 5 periods of 6 days each, and 5 intermediate days, namely, 1 day at the commencement of each period of test :—

1st period.	Chronometer at temperature of		55°	F. or	13°	C.
2nd „	„	„	70°	„	21°	„
3rd „	„	„	85°	„	29°	„
4th „	„	„	70°	„	21°	„
5th „	„	„	55°	„	13°	„

Certificates are granted to chronometers which have undergone 35 days' test as specified above, and whose performance is such that:—

1. The mean of the differences in each stage of the examination, between (*a*) the average daily rate during that period, and (*b*) the several daily rates, does not exceed one second in any one of the stages.

2. The mean daily rate has not been affected by change of temperature more than one-sixth of a second per 1° F., which is about a quarter of a second per 1° C.

3. The mean daily rate has not exceeded five seconds in any stage of the test.

The trials were commenced in August, and up to present date seventeen ordinary marine and one sidereal chronometer have been rated.

The Astronomer Royal having on enquiry certified as to the excellent working of Kullberg's temperature regulator in the chronometer oven at the Royal Observatory, Greenwich, the inventor has been instructed to fit a similar one to the Kew testing case.

A Richard Thermograph has also been procured, and is arranged to work in the case with the chronometers, so as to afford a continuous

record of the temperatures which they have experienced during the whole of their trial.

The range of temperature from 55° to 85° F., to which the marine chronometers are submitted, has been decided upon after careful consideration, as being amply sufficient for determining the behaviour of chronometers under conditions to which they are usually exposed at sea, and no objections have yet been received from makers or others to the adoption of the above range.

VII. Miscellaneous.

Photographic Paper, &c.—This has been supplied to the Observatories at Batavia, Coimbra, Falmouth, Glasgow, Lisbon, Mauritius, Oxford, Stonyhurst, St. Petersburg, and Toronto, and to the Meteorological Office. Blank forms have also been supplied to various Observatories and individuals.

At the request of Senhor Capello, of the Lisbon Observatory, an astronomical clock was procured and shipped to the Loanda Observatory, for use during the recent solar eclipse.

Two barograph tabulators, photographic appliances, and various other instruments have been procured, verified, and forwarded to the Observatories at Hong Kong and Mauritius.

The Observatory has been presented by the Rev. John Rigaud, B.D., Fellow of Magdalen College, Oxford, with a bust of his father, Stephen Peter Rigaud, Esq., M.A., F.R.S., Savilian Professor of Astronomy and Radcliffe Observer, who formerly assisted his uncle, the Rev. S. Demainbray, in carrying on the Observatory.

Exhibitions, &c.—At the request of the Council of the Royal Meteorological Society a number of old instruments were exhibited at the Exhibition held by the Society in the rooms of the Institution of Civil Engineers in March, and devoted this year to barometers.

Four sets of photographs illustrative of the various processes in use at different periods at the Observatory have been contributed to the Photographic Exhibition, held in the Corporation Galleries of Art at Glasgow.

Library.—In July the Superintendent received a letter from the Secretary of the Royal Society offering a number of duplicate volumes about to be removed from the Library at Burlington House, and forwarding a catalogue.

A selection was made of those suitable for the Observatory Library, and sixteen volumes were accordingly sent down to Kew.

Presents of publications were received during the year from—

34 Scientific Societies and Institutions of Great Britain and Ireland, and

92 Foreign and Colonial Scientific Establishments, as well as numerous private individuals.

Magnetic Reductions.—At the request of Professor Balfour Stewart, the Superintendent prepared and submitted to the Committee of the British Association on the Reduction of Magnetic Observations, a report on the comparison between Wild's, Sabine's, and the Greenwich methods of determining the solar diurnal range of the declination.

Workshop.—The machine tools procured for the use of the Kew Observatory by grants from the Government Grant Fund or the Donation Fund have been kept in thorough order.

House, Grounds, and Footpath.—These have all been kept in order during the year.

Her Majesty's Commissioners of Woods and Forests have kindly complied with the request of the President and Council of the Royal Society that the Observatory Staff should have a free passage at all hours through the yard tenanted by the lessee of the Old Deer Park, and accordingly an iron turnstile has been erected at the expense of the Committee at the entrance gate to the Park.

The necessary external repairs and painting of the building have been carried out by Her Majesty's Commissioners of Works as usual.

Owing to the increase of work now undertaken by the Observatory Staff it has become necessary to consider means of increasing the available accommodation, and of providing more space by addition either to the Observatory building itself or to one of the outbuildings.

Plans for both schemes have been submitted to the Committee, together with estimates of the approximate cost.

Personal Establishment.

The staff employed is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, Chief Assistant and Magnetic Observer.
H. McLaughlin, Librarian and Accountant.
E. G. Constable, Solar Observations and Rating.
W. Hugo, J. Foster, T. Gunter, W. Boxall, } Verification Department.
E. Dagwell.
H. A. Widdowson.
F. Oliver.
W. C. Gough.
E. Redding.
M. Baker, Messenger and Care-taker.

Abstract. The Kew Observatory. Account of Receipts and Payments for the year ending October 31st, 1886.

Dr. **RECEIPTS.**

	£	s.	d.
To Balance from 1884–85	416	18	9
Royal Society (Gassiot Trust)	489	17	7
Meteorological Office (Allowance)	400	0	0
Experimental Work	96	9	9
Magnetic Work	6	10	8
Miscellaneous	81	18	1
Verifications	729	5	5
Rating of Watches and Chronometers	343	2	3
	2564	2	6
Meteorological Office for Postages and Porterages	7	17	0
Commissions executed for Colonial and Foreign Institutions, &c.	525	4	6
	£3097	4	0

PAYMENTS. *Cr.*

	£	s.	d.
Salaries and Extra	1328	6	0
Gas, Fuel, Rent, Insurance, &c.	169	13	0
Stationery, Postages, Caretaker, &c.	141	3	11
Instruments, Apparatus, &c.	61	16	3
Experimental work	56	17	1
International Circumpolar Committee	2	6	0
Pendulum	11	9	2
Magnetic	24	7	7
Miscellaneous	43	5	3
Exhibition	7	11	6
Verifications	131	9	5
Rating of Watches and Chronometers	141	14	0
	2119	19	2
Postages and Porterages, &c., for Meteorological Office	25	6	11
Commissions executed for Colonial and Foreign Institutions, &c.	375	3	5
Balance—Bank of England	493	8	11
London and County Bank	69	0	0
Cash in hand	14	5	7
	576	14	6
	£3097	4	0

November 19, 1886. Examined and compared with the Vouchers, and found correct. (Signed) J. T. WALKER, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	576	14	6
Meteorological Office, Allowances, Experimental, and Sundries	95	6	4
Verification Fees due, &c.	65	7	2
Rating of Watches, &c., due	18	1	6
Photographic Paper	20	0	0
Commissions, &c.	89	3	6
Blank Forms	7	0	9
Standard Thermometers	91	18	0
Magnetic Pendulum, &c.	38	5	11
	£1001	17	8

November 19, 1886. (Signed) FRANCIS GALTON, *for Chairman.*

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	9	8	5
Stationery, &c.	3	16	5
Rating	13	16	3
Instruments, Chemicals, &c.	3	10	7
Commissions, including Grant on account for the Hong Kong Observatory	165	13	0
Balance	805	13	0
	£1001	17	8

(Signed) G. M. WHIPPLE, *Superintendent.*

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* 0^h 1^m 15^s·1 *W., for the year October* 1885 *to September* 1886.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets 101 B and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are 3½ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log $\pi^2 K$ employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000{,}000{,}213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is −0·00148.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have been made and reduced by Mr. T. W. Baker.

Table I.

Observations of Inclination.

Month.	Mean inclination.	Month.	Mean inclination.
1885.		1886.	
October 26.....	67° 37′·8	April 27.....	67° 36′·8
27.....	67 37·8	29.....	67 37·2
Mean.....	67 37·8	Mean.....	67 37·0
November 23.....	67 37·1	May 25.....	67 38·0
24.....	67 37·3	26.....	67 35·7
28.....	67 37·5	Mean.....	67 36·8
Mean.....	67 37·3	June 24.....	67 36·2
December 22.....	67 37·6	26.....	67 36·1
23.....	67 38·2	Mean.....	67 36·2
24.....	67 37·7	July 21.....	67 37·7
Mean.....	67 37·8	22.....	67 37·2
1886.		Mean.....	67 37·5
January 26.....	67 37·8	August 23.....	67 35·7
27.....	67 37·0	24.....	67 37·5
Mean.....	67 37·4	Mean.....	67 36·6
February 22.....	67 36·4	September 23.....	67 36·3
23.....	67 37·3	29.....	67 36·2
26.....	67 36·4	Mean.....	67 36·3
Mean.....	67 36·7		
March 22.....	67 38·7		
27.....	67 37·0		
29.....	67 37·0		
Mean.....	67 37·6		

Table II.

Observations for the Absolute Measure of Horizontal Force.

Month.	Log $\frac{m}{X}$ mean.	Log mX mean.	Value of m*.
1885.			
October 29th and 30th ..	9·12250	0·30825	0·51925
November 26th	9·12195	0·30872	0·51919
December 29th.....	9·12187	0·30874	0·51916
1886.			
January 28th.....	9·12154	0·30902	0·51913
February 24th	9·12184	0·30923	0·51943
March 25th	9·12221	0·30860	0·51928
April 1st and 2nd	9·12230	0·30865	0·51937
„ 28th	9·12086	0·30800	0·51812
May 27th	9·12103	0·30932	0·51901
June 28th	9·12095	0·30836	0·51838
July 23rd	9·12171	0·30807	0·51867
August 26th	9·12193	0·30775	0·51861
September 27th	9·12122	0·30823	0·51847

Table III.—Solar Diurnal Range of the Declination.

Hour.	Summer mean.	Winter mean.	Annual mean.
0	+5′·6	+2′·9	+4′·3
1	+6·8	+4·8	+5·8
2	+6·0	+4·4	+5·2
3	+5·0	+3·5	+4·3
4	+2·8	+1·5	+2·2
5	+1·8	+0·6	+1·2
6	+0·7	−0·5	+0·1
7	−0·2	−0·4	−0·3
8	−0·3	−1·0	−0·7
9	−0·3	−0·9	−0·6
10	−0·9	−1·3	−1·1
11	−0·7	−1·3	−1·0
12	−1·0	−1·3	−1·2
13	−1·2	−1·1	−1·2
14	−2·0	−0·8	−1·4
15	−2·2	−0·4	−1·3
16	−2·5	−0·8	−1·7
17	−3·0	−0·7	−1·9
18	−3·8	−0·9	−2·4
19	−4·0	−1·3	−2·7
20	−4·2	−1·5	−2·9
21	−3·5	−2·1	−2·8
22	−1·5	−1·7	−1·6
23	+1·8	+0·8	+1·3

* m = moment of vibrating magnet.

Table IV.

Month.	Declination.	Magnetic Intensity.								
		English Units.			Metric Units.			C. G. S. Measure.		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1885.										
October	18° 23′ 34″	3·9163	9·5161	10·2903	1·8058	4·3877	4·7447	0·1806	0·4388	0·4745
November	18 20 1	3·9209	9·5227	10·2984	1·8078	4·3908	4·7484	0·1808	0·4391	0·4748
December	18 18 1	3·9213	9·5282	10·3036	1·8080	4·3933	4·7508	0·1808	0·4393	0·4751
1886.										
January	18 20 19	3·9241	9·5315	10·3077	1·8093	4·3948	4·7527	0·1809	0·4395	0·4753
February.....	18 17 45	3·9237	9·5251	10·3017	1·8092	4·3919	4·7500	0·1809	0·4392	0·4750
March	18 20 36	3·9192	9·5212	10·2963	1·8071	4·3901	4·7474	0·1807	0·4390	0·4747
April	18 19 9	3·9208	9·5201	10·2960	1·8078	4·3896	4·7473	0·1808	0·4390	0·4747
May.....	18 19 51	3·9277	9·5361	10·3134	1·8110	4·3969	4·7553	0·1811	0·4397	0·4755
June	18 17 47	3·9237	9·5209	10·2977	1·8092	4·3900	4·7481	0·1809	0·4390	0·4748
July.....	18 17 59	3·9190	9·5198	10·2949	1·8070	4·3894	4·7468	0·1807	0·4389	0·4747
August	18 15 28	3·9166	9·5071	10·2823	1·8059	4·3836	4·7410	0·1806	0·4384	0·4741
September	18 16 11	3·9219	9·5172	10·2937	1·8083	4·3882	4·7462	0·1808	0·4388	0·4746

APPENDIX II.

Meteorological Observations.—Table I.

APPENDIX II.

Meteorological Observations.—Table I.

Mean Monthly results.

Months.	Thermometer.* Mean.	Means of— Max.	Min.	Max. and Min.	Absolute Extremes. Max.	Date.	Min.	Date.	Barometer.† Mean.	Absolute Extremes. Max.	Date.	Min.	Date.	Mean vapour-tension.
1885.						d. h.		d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.....	46°5	51°7	40°9	46°3	58°9	2 2 P.M.	32°8	30 7 A.M.	29·718	30·206	17 9 A.M.	28·955	10 7 A.M.	·257
Nov. ...	43·9	48·1	39·0	43·6	58·4	30 11 A.M.	31·1	16 3 ,,	29·912	30·415	16 9 P.M.	29·250	26 Midt.	·247
Dec. ...	38·7	42·7	34·1	38·4	50·4	3 8 P.M.	23·2	11 7 ,,	30·223	30·600	23 {10 & 11 A.M.}	29·263	6 1 A.M.	·202
1886.														
Jan.....	36·3	40·5	31·3	35·9	51·3	2 3 ,,	15·8	7 11 P.M.	29·675	30·166	12 10 ,,	29·007	18 4 ,,	·183
Feb. ...	33·9	37·5	30·5	34·0	47·5	13 3 ,,	22·3	10 {7 & 9 A.M.}	30·139	30·759	8 10 P.M.	29·246	1 7 ,,	·163
March..	39·9	46·1	34·6	40·4	64·4	24 3 ,,	22·2	17 6 ,,	29·982	30·426	{9 8 A.M. 11 1 ,,}	29·257	2 {8 P.M. & Midt.}	·197
April...	46·3	53·7	40·0	46·9	64·8	24 2 ,,	33·8	11 3 ,,	29·928	30·340	13 10 P.M.	29·209	8 3 P.M.	·239
May ...	52·5	60·8	44·4	52·6	71·9	{7 3 ,, 8 2 ,,}	30·9	1 4 ,,	29·942	30·501	5 8 A.M.	29·101	13 9 A.M.	·296
June...	57·0	65·6	49·0	57·3	75·2	29 4 ,,	40·3	5 4 ,,	29·992	30·305	30 Midt.	29·710	12 2 P.M.	·329
July ...	62·2	70·8	53·5	62·2	83·2	4 5 ,,	46·6	28 5 ,,	29·929	30·326	3 7 A.M.	29·366	26 2 A.M.	·397
Aug....	61·8‡	70·9	53·9	62·4	84·4	{30 2 ,, 31 ?}	44·1	3 5 ,,	29·999	30·299	20 7 ,,	29·514	13 3 P.M.	·415
Sept....	58·4§	66·3	50·6	58·5	80·9	1 ?	41·2	18 6 ,,	30·040	30·491	15 Midt.	29·616	21 {1 & 3 P.M.}	·372
Means..	48·1	54·6	41·8	48·2	..	....	..	....	29·957	..	....	..	....	·275

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council.

* The thermometers are 10 feet above the ground.

† Reduced to 32° at M.S.L.

‡ Mean for one day is approximate.

§ Means for three days are approximate.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall *.			Weather. Number of days on which were registered							Wind †. Number of days on which it was									
		Total.	Maximum.	Date.	Rain.	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Variable.	Calm.
1885.		in.	in.																		
October ..	7·1	3·865	0·905	23	20	..	..	..	3	18	..	5	2	2	2	1	7	6	4	2	2
November	7·5	2·970	0·555	24	16	..	..	..	3	17	1	2	7	7	2	1	6	1	1	3	4
December	6·6	1·165	0·350	5	14	1	..	..	7	17	1	5	3	2	1	3	10	3	2	2	8
1886.																					
January ..	6·5	3·510	0·685	5	24	11	1	..	3	13	..	3	1	1	2	3	7	4	5	5	3
February .	7·7	0·670	0·270	2	8	3	..	..	5	17	..	6	8	1	2	2	2	2	2	3	9
March ...	6·9	1·350	0·280	20	12	3	..	..	5	14	3	2	5	6	1	3	9	2	1	2	2
April	6·3	1·495	0·310	28	12	3	1	1	5	12	..	3	9	3	..	2	7	5	1	2	2
May	6·8	4·100	1·200	12	19	..	..	2	5	13	1	1	4	4	1	4	9	5	1	6	7
June	6·3	0·845	0·440	10	10	..	..	1	5	10	..	6	4	4	..	1	4	1	3	4	3
July	5·9	2·380	0·510	25	15	..	..	1	7	11	..	2	..	2	1	5	8	3	3	5	3
August ..	5·7	0·675	0·150	1	10	..	..	..	4	7	..	3	2	1	1	4	8	1	3	4	7
Septembe·	5·8	1·790	0·385	10	11	..	..	1	6	9	..	5	5	3	1	4	7	4	1	3	4
Totals..		24·815			171	21	2	6	58	158	6	43	50	36	14	33	84	37	27	41	54

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the Air.*		
	Total number of hours recorded.	Percentage of possible sunshine	Greatest daily record.	Date.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average hourly Velocity.	Greatest hourly Velocity.	Date.
1885.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
October	93 12	29	9 36	7	93	113	1	36	24·8	30	11	32	31
November	39 6	14	7 6	1	66	95	2	35	18·9	16	11	38	28
December	48 36	20	5 54	10	58	79	1	29	14·3	11	9	38	28
1886.													
January	47 0	18	6 18	16	64	89	28	27	10·9	8	11	33	16
February	38 12	14	6 48	1	59	86	26	26	15·1	9	8	27	1
March	72 48	20	9 12	9	86	114	31	30	14·5	16	14	40	30 31
April	151 0	36	11 6	23	106	121	6	36	25·0	10	15	34	8
May	169 0	35	12 54	4	114	137	8	40	19·4	1	10	38	18
June	222 30	45	13 54	4	124	138	27	45	36·0	17	9	23	19 20 23
July	211 0	42	14 54	5	129	142	7	49†	39·9	11	9	33	22
August	189 12	42	11 54	3	125	138	3	50	37·1	3	8	26	13
September	134 18	35	11 12	7	114	130	1	47	32·4	17	10	30	9 27

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground. † Approximate mean for 21 days only.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days without spots.
1885.			
October	15	6	2
November	7	4	0
December	13	5	1
1886.			
January	13	3	3
February	8	4	0
March	15	9	0
April	17	9	0
May	12	3	2
June	18	9	1
July	17	9	1
August	17	6	3
September	17	6	2
Totals	169	73	15

APPENDIX III.

Results of Watch Trials. Performance of 20 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, &c.	Mean daily rate. — +Gaining. −Losing.	Mean variation of daily rate.	Mean change of rate for 1° F.	Difference between pendant up and dial up.	Difference of mean daily rate: Between pendant up and pendant right.	Between pendant up and pendant left.	Between dial up and dial down.	Difference between extremes of daily rate.	Marks awarded for: Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	Total Marks. 0—100.	Character of test.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.					
E. F. Ashley, Clerkenwell ...	03385	Overcoil, fusee	+0·5	0·3	0·01	−0·9	−0·8	+1·9	−3·7	7·75	33·3	34·1	19·3	86·7	Class A.
Baume & Co., London.....	2163	Overcoil, going barrel	−2·1	0·5	0·03	+3·4	−0·4	+0·2	−3·7	4·75	30·5	35·6	18·0	84·1	,,
Parkinson and Frodsham, London	6660	Duo-in-Uno, fusee.....	−0·1	0·4	0·07	−1·1	+1·8	−0·4	+1·3	5·75	31·2	37·1	15·3	83·6	,,
Baume & Co., London.....	2160	Overcoil, going barrel	+0·2	0·4	0·03	−2·2	−4·7	−0·5	+3·6	7·25	32·6	32·9	17·7	83·2	,,
Stauffer & Co., London	114322	Overcoil, going barrel	+2·0	0·4	0·05	+1·6	−2·6	+3·2	+1·3	7·75	32·2	32·5	16·5	81·2	,,
Baume & Co., London.....	2272*	Overcoil, going barrel	+3·3	0·7	0·01	+2·0	+2·0	−0·5	−0·2	6·25	25·9	35·8	19·3	81·0	,,
Baume & Co., London.....	2161	Overcoil, going barrel	+1·7	0·3	0·06	+2·4	−3·0	−4·4	−0·5	7·25	34·1	30·2	16·3	80·6	,,
D. Buckney, London	12925	Overcoil, going barrel	−1·2	0·6	C·08	+0·9	+0·7	+1·5	+0·4	7·5	27·5	38·4	14·3	80·2	,,
D. Keys, London	15317	Overcoil, going barrel	−3·6	0·6	0·07	+2·2	−2·2	+0·6	−2·4	5·25	28·6	35·8	15·6	80·0	,,
Baume and Co.	2159	Overcoil, going barrel	−2·7	0·5	0·02	+3·7	−2·3	+1·5	+0·4	8·25	29·0	31·8	18·9	79·7	,,
Holland, Rockferry	3576	Overcoil, fusee	−1·0	0·5	0·06	+4·1	+0·8	+4·8	−0·4	7·5	30·8	32·7	16·0	79·5	,,
Stauffer & Co.	111115	Overcoil, going barrel	−0·4	0·5	0·05	+0·6	−3·8	−3·9	−0·9	6·75	30·2	32·5	16·8	79·5	,,
Manger & Co., London	6001	Overcoil, going barrel	−1·8	0·6	0·03	+0·6	−2·0	+2·1	+2·5	8·25	27·4	34·0	18·0	79·4	,,
Carley & Co., London	44568	Overcoil, going barrel	+3·1	0·6	0·05	+2·4	+1·4	−2·0	−1·2	6·5	27·6	35·0	16·7	79·3	,,
Stauffer & Co.	114318	Overcoil, going barrel	−3·1	0·5	0·10	0·0	−2·4	+1·8	+0·5	7·75	29·5	36·2	13·4	79·1	,,
Player, Coventry	13491	Overcoil, fusee	−2·4	0·5	0·10	+1·3	+0·5	−2·9	−0·1	6·75	30·4	35·3	13·3	79·0	,,
Usher & Cole, London..	24044	Overcoil, going barrel	−0·8	0·7	0·04	−1·4	−2·9	−4·1	−2·4	7·5	27·0	34·4	17·5	78·9	,,
H. Golay, London.....	14693†	Overcoil, going barrel	+1·3	0·5	0·09	+1·3	−0·4	+3·7	+1·1	7·5	30·0	34·7	14·0	78·7	,,
D. Buckney, London	12485	Overcoil, going barrel	−0·7	0·6	0·08	+3·3	+3·5	−1·3	+2·0	6·0	28·4	35·4	14·8	78·6	,,
Baume & Co.	2343	Overcoil, going barrel	−0·5	0·6	0·07	−1·5	−2·8	−3·8	+1·3	6·75	28·4	34·8	15·3	78·5	,,

* Fly-back chronograph. † Minute repeater and clock watch.

APPENDIX IV.

List of Instruments, Apparatus, &c., the Property of the Kew Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Old Kew Thermometer Screen	1868
	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo-Heliograph, Pendulum Apparatus, Dip-Circle, Unifilar, and Hodgkinson's Actinometer.	1876
Dr. T. Thorpe, F.R.S.	Three Open Scale Standard Thermometers, Nos. 561, 562, and 563.	1879
	Tripod Stand	1883
Lieutenant A. Gordon, R.N.	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. One Bifilar Magnetometer. One Declinometer. Two Tripod Stands.	1883
General Sir H. Lefroy, R.A., F.R.S.	Toronto Daily Registers for 1850–3	1885
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.	1883
Professor O. J. Lodge	Unifilar Magnetometer, by Jones, No. 106, complete. Barrow Dip-Circle, No. 23, with two Needles, and Magnetizing Bars. Tripod Stand.	1883
Mr. W. F. Harrison.	Condensing lens and copper lamp chimney ..	1883
Captain W. de W. Abney, F.R.S.	Mason's Hygrometer, by Jones	1885
Professor Rücker ...	Tripod stand	1886

"The Minute Anatomy of the Brachial Plexus." By W. P. HERRINGHAM, M.B., M.R.C.P. Communicated by W. S. SAVORY, F.R.S. Received March 8,—Read March 25, 1886.

It has for some time appeared probable that the spinal nerves which form the brachial plexus do not become confounded one with another, but retain each its separate course and its separate functions.

To the naked eye a nerve is a bundle of parallel threads bound together, and at the same time divided by a sheath of connective tissue. It seemed to me possible that the course of the spinal nerve roots could be traced by a dissection which should follow each through the plexus to the nerves which branch therefrom, and in these to its final destination.

My dissections were partly upon fœtuses or stillborn children, partly upon the adult. The plexus of an infant is in some respects better, in some worse than that of an adult for an investigation of this kind. On the one hand its minuteness needs the confirmation of larger tissues, but on the other hand the fibrous sheath uniting the nerves is very much weaker and less perfect, so that as in the case of the pectorals, the serratus magnus, and coraco-brachialis muscles, arrangements which in the adult need great care in dissection, can be seen in the infant without using the knife at all.

Adult bodies are best fitted for tracing fibres down a long nerve.

The present paper is based upon the dissection of fifty-five plexuses, thirty-two being fœtal or infantile, and twenty-three adult.

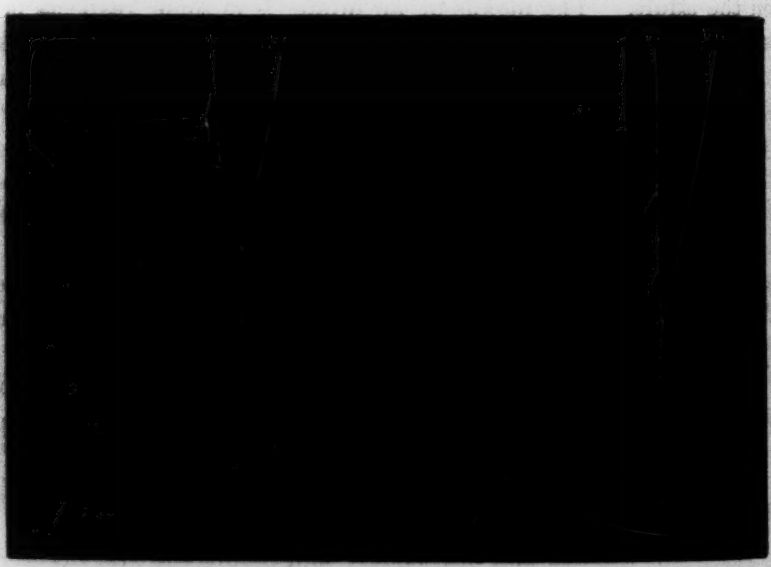
The 5th cervical nerve as it lies between the scaleni gives off a branch which divides into the nerve to the rhomboids, and the upper root of the *posterior thoracic*. The latter is joined by the root from the 6th, and lower down by one or sometimes two branches from the 7th.

In the adult the 5th and 6th usually join before the first digitations of the serratus magnus are reached, and receive the 7th about the level of the first rib. It can generally, even in adults, be seen that the 7th joins the nerve below the twig given to the first part of the muscle formed by the two upper digitations, and it is often easy to separate the 5th from the 6th, and this from the 7th, so far as to show that the first part of the muscle is supplied wholly by the 5th, and the second by the 6th alone, or by the 5th and 6th, while the 7th does not give twigs until the third part is reached. I have several times been able to show this to students in the dissecting room. But in the fœtus there is no dissection necessary. The branch to the first part is given off before the 5th reaches the 6th, and the 7th does not enter

the nerve until it has descended to the third or lowest part of the muscle. The connective tissue which in the adult binds all the nerve together has not in the fœtus yet grown up, and the system of the nerve is naturally exposed. The supply of each nerve is variable. If the 5th root is large it will send on fibres to supply with the 6th the middle of the muscle. If it is small it spends itself entirely on the upper part, but however the different roots may vary in size I have never in the numerous dissections I have made seen them alter their relative positions.

Two typical examples from adults may be quoted. In one, the 5th gave twigs to the first three digitations, and then a small fibre to the 6th. There was no root from the 7th. In the other, the 5th supplied the first two, the 6th alone supplied the next two, and the 6th and 7th the remaining digitations. In these there was no splitting of the nerve necessary. The drawings shown were made from them.

Diagrammatic Sketch of Posterior Thoracic from two Adults.



V, VI, VII = Branches from 5th, 6th, and 7th roots.
1, 2, 3 = First, second, and third parts of serratus magnus.

As the 5th leaves the scaleni it receives a communicating branch from the 4th, which therefore does not enter the rhomboid branch, or the posterior thoracic. The communicating branch varies considerably in size. I have at times been scarcely able to recognise the twig, at others it is a sixteenth of an inch thick. In the remainder of the paper this branch is not separated from the 5th.

Soon after this the 5th root joins with the 6th. It is usually at their junction that the *suprascapular* nerve is given off, but it is not

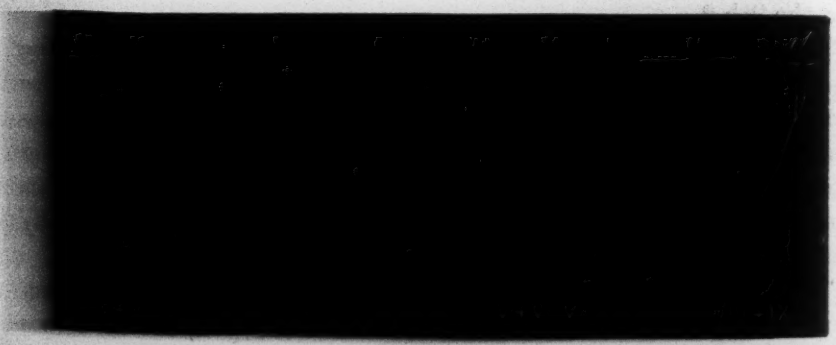
uncommon to find it springing from the 5th before the junction is made. When this is the case it contains no fibres from the 6th, but when given off at the junction it usually, though not always, receives a minute fibre from the 6th which passes either behind or through the fibres of the 5th to reach the suprascapular. The 6th therefore exercises sometimes an extremely small influence, and sometimes none at all over the supra- and infra-spinatus.

The united 5th and 6th then divide into an anterior and a posterior branch.*

The anterior joins with an anterior branch of the 7th to form the outer cord. Just before their junction the united 5th and 6th gives off one, and the 7th two branches for the pectorals. The upper branch from the 7th joins with that from the 5th and 6th to form the *external anterior thoracic*, which, piercing the costo-coracoid membrane, supplies the clavicular and the upper piece of the sternal part of the muscle as far down as the fibres which arise from the 2nd costal cartilage. The lower 7th branch forms that which is usually called the *communicating branch* of the external anterior thoracic. It has, however, a separate origin from the 7th trunk, and contains no fibre of the 5th or

Diagram of Anterior Thoracics.

Fig. 3.—As seen in Adults. Fig. 4.—As seen in a Fœtus.



V, VI, VII, VIII, IX = 5th, 6th, 7th, 8th cervical and 1st dorsal roots.
V + VI, VIII + IX = Trunks formed by union of these roots.
a = Twigs to clavicular division of pectoralis major.
b, *c*, *d* = Twigs to upper, middle, and lower parts of the sternal division of the muscle.
X = "Communicating branch," of which X_1 goes over, X_2 under, the pectoralis minor.

6th. It runs under the costo-coracoid membrane over the axillary artery, and at the upper border of the pectoralis minor divides into two, of which one branch passes in front, running with the branches

* Or the nerves may each divide before they join.

of the acromial thoracic artery, to supply the pectoralis major; the other goes behind the pectoralis minor, sends two twigs through it which supply it and the next parts of the pectoralis major, and close to the lower border of the muscle joins with the *internal anterior thoracic* which appears from under the artery. The united nerve now supplies the lowest part of the pectoralis minor, and turning round its edge serves the lowest fibres of the pectoralis major.

This arrangement was constant in eight consecutive adult cases that I dissected. I was, however, able in one of the fœtal dissections to see more than this. The branch from the 5th and 6th, before joining the branch from the 7th, gave a twig which ran to the clavicular portion and which alone supplied that part. The union of the 5th, 6th, and 7th supplied the upper part of the sternal muscle. The 7th served the middle as in the adult, but the internal anterior thoracic before receiving the 7th gave off a twig which supplied the lowest fibres of the pectoralis major, its union with the 7th serving the fibre next in order above these.

Of the nerves interested in the anterior thoracics the 7th always contributes. Of thirteen dissections the 5th and 6th both gave branches to the external in five, the 6th alone in eight cases. Of ten dissections the 8th and 9th both gave branches to the internal in eight cases, the 8th alone in two. It appears, therefore, that the pectoralis major does not usually receive from the 5th, and does usually receive from the 9th.*

Below these branches the 7th joins the 5th and 6th, thus forming the outer cord, which bifurcates into the musculo-cutaneous and the outer head of the median. I have seen three cases where the musculo-cutaneous when about to pass under the biceps to the outer side of the arm gave back a large bundle to the median. Two of these cases were adult, and in them the returned bundle consisted in one wholly of the 6th root, in the other of fibres from both 6th and 7th.

The *musculo-cutaneous* is usually described as supplying the coraco-brachialis. In the adult the nerve to this muscle is given off before the musculo-cutaneous enters it. If traced up it is seen to come from the outer cord before the musculo-cutaneous leaves it, and ultimately from the 7th root in the outer cord. It passes under the cord to get to the muscle.

Here again the system is very much clearer in the fœtus, for in the fœtal plexus the coraco-brachial nerve can be seen running from the 7th before it joins the 5th and 6th, and passing under the 5th and

* The first dorsal I have here called, and shall henceforth call, the 9th spinal root, the 2nd and 3rd dorsal the 10th and 11th respectively. The 9th receives at its origin a small branch from the 10th in the majority of cases. It is too small to dissect in the fœtus, and the conditions of a dissecting room do not allow of its dissection in the adult. I have therefore included it in the 9th root.

6th to reach the muscle. It is therefore quite distinct not only from the musculo-cutaneous, but from the 5th and 6th also. I have found this arrangement constant in the fœtus in more than twelve consecutive cases, in fact ever since I first recognised it. I have also seen it in the adult.

In one case, however, I found the muscle supplied not only by this nerve from the 7th, but also by a fibre from the 6th in the musculo-cutaneous.

The rest of the musculo-cutaneous, or as should be said, the musculo-cutaneous nerve proper, hardly ever contains any fibres other than those of the 5th and 6th. In thirty-nine cases, twenty-one fœtal and eighteen adult, the 7th only contributed to the musculo-cutaneous four times, twice in the fœtus, and twice in the adult. Of these four exceptions, in two the 7th could not be traced down the nerve, in a third it ran to the short head of the biceps, and in the fourth, an adult case, it entered the cutaneous branch only.

Twenty-eight cases were examined to see if both 5th and 6th entered the nerve. In twenty-seven this was found to be the fact, in one, only the 5th was traced into it.

In eight cases the 5th and 6th were traced down the nerve. Four of these were fœtal, and four were adult. The biceps was found to be served by both 5th and 6th in seven instances, in one the 5th alone was traced to the muscle. The brachialis anticus was supplied by both the 5th and 6th in four cases, and by the 5th alone in four. The cutaneous branch was found to contain fibres from both roots in six cases; in one it was formed by the 6th and 7th. The bundle from the 5th was noted as very small in two cases, and in two more was found to supply the skin over the outer condyle and the head of the radius, the nerve in the forearm being wholly derived from the 6th root. In another case it ran down the forearm over the flexor carpi radialis in front of the branches from the 6th.

The remaining nerve given from the outer cord is the outer head of the median. Into this the 5th does not enter. Thirty-one dissections showed no exception to this rule. The supply of the 5th by its anterior branch ends therefore with the musculo-cutaneous nerve.

The *median* is formed by two heads; into the outer the 6th and 7th always enter, while the inner is formed always by branches of the 8th and 9th, sometimes with the addition of some bundles of the 7th. This variety depends upon whether the anterior branch of the 7th bifurcates or goes wholly to the outer cord. In order to see whether both 8th and 9th contribute to the median, twenty-eight dissections were made, fourteen in infants, fourteen in adults. In one fœtus, and in one adult, no branch from the 9th was found. These two were, however, the only exceptions to the rule that both roots send fibres to the nerve. The median then is made of the 6th, 7th, 8th,

and 9th. But these roots do not send to it a constant proportion. There is little variation in the size of the 6th bundle, the 7th varies considerably, the 8th is sometimes equal to, sometimes smaller than, and sometimes larger than the 9th.

The 6th bundle runs down the outer side of the nerve from the top to the bottom, though in dissection the nerve generally becomes so twisted that it seems otherwise. In the lower third of the arm a pair of fibres are seen crossing from the outer to the inner side. These are the nerves to the pronator teres and the flexor carpi radialis. They are given off almost always by the 6th, and they usually run over in front of the nerve to reach the muscles.

I have, however, dissected them running through the nerve and separating the other bundles. The nerve to the pronator usually divides into two before it reaches the muscle, and is sometimes double from the beginning. With the nerve to the flexor carpi radialis runs a nerve to the other flexors, but this comes from a lower root. In eight cases where the 6th and 7th were not separated, these two muscles were supplied by the combined bundle. The nerve to the pronator teres was traced seventeen times to the 6th, once to the 7th; that to the flexor carpi radialis thirteen times to the 6th, twice to the 7th; neither to any other nerve. The 6th does not supply any other muscles in the forearm.

The 7th was traced nine times to the flexor sublimis, and in four of these it also contributed to the anterior interosseous. It is to be noted that in two of these four it also formed part of the ulnar. Of the five cases when it did not go to the anterior interosseous the ulnar was traced in four, and did not in any of these receive from the 7th. Where then the 7th goes to the anterior interosseous it is probable that it will form also part of the ulnar; when it is excluded from the ulnar it probably does not contribute to the anterior interosseous. In seven cases no branch could be traced to the flexor sublimis.

The 8th and 9th usually supply the flexor sublimis, and always the deep flexors. In seventeen instances the flexor sublimis received from them fourteen times, and three times did not receive from them. The two nerves were separated six times in adults, with the result that they seemed to mingle in all muscular branches in the forearm, and that no muscle could be said to belong to the one and not to the other.

After the forearm muscles have been supplied, the remainder of the median which comes from under the flexor sublimis always contains fibres from the 6th, 7th, and 8th roots, and sometimes a bundle from the 9th.

The 6th was traced separately eleven times, and the 7th seven times in this part of the nerve. Neither was ever found absent. The distribution of the lower two roots in the hand was traced in eleven cases. In six of these they were not separated; in the other five the

9th ran to the hand in two only; in three of these, and in one other case, it was seen to end in the muscles of the forearm. Of these six cases two were fœtal, the rest adult. The 8th always ran to the hand. As a rule then the median in the hand contains only fibres from the 6th, 7th, and 8th.

The palmar cutaneous branch was five times noted, twice coming from the 6th, three times from the 7th.

The branch to the superficial muscles of the thumb was eleven times dissected. In eight cases it came from the 6th, in three from the undivided 6th and 7th, never from the 8th or 9th.

The five digital branches* were more variously distributed. The first branch was traced fourteen times, nine times to the 6th, and five times to the united 6th and 7th.

The second was traced fourteen times, six times to the 6th, five times to a united 6th and 7th, twice to both 6th and 7th, and once to the 7th alone. The third was traced eleven times, twice to the 6th, five times to a united 6th and 7th, once to both 6th and 7th, and thrice to the 7th. The fourth was traced eleven times, once to a united 6th and 7th, three times to the 7th, twice to the 8th, once to a united 8th and 9th, and in four the radial side of the cleft was supplied by the 7th or the united 6th and 7th, and the ulnar side by the lower nerves. The 5th was traced eleven times; in one the 7th served the radial side of the cleft, the other side being supplied by the united 8th and 9th, in three the 8th alone, in five the undivided 8th and 9th, in one both 8th and 9th (the latter taking the ulnar half), and in one the 9th alone supplied it.

These figures are confusing to read, but the table shows that the first branch is always supplied by the highest nerve, the last always from one of the lowest, and that in no case is there a break in the order of supply from the one to the other.

No. of case.	1.	2.	3.	4.	5.	13.
Digital branches.						
1.....	6+7†	6	6+7	6+7	6	6
2.....		6			7	6
3.....		7			7	..
4.....	7, 8+9	7, 8+9	8	6+7, 8+9	8+9	..
5.....	8+9	8+9	9	8+9	8+9	..

* 1st, radial side of thumb; 2nd, ulnar ditto; 3rd, radial of index; 4th, cleft between index and 3rd finger; 5th, cleft betwten 3rd and 4th finger.

† The sign + means that the two nerves were not separated. In Case 1 the first three branches arose from the bundle formed by the union of the 6th and 7th, the fourth had two roots, one from the 7th alone, the other from the bundle formed by the union of the 8th and 9th.

No. of case.	14.	15.	23.	42.	43.	52.	53.	54.
Digital branches.								
1.....	6	6	6	6	6	6	6+7	6+7
2.....	6	6	6	6	6,7	6,7		
3.....	..	..	6	6	6,7	7		
4.....	..	..	7	7	8	7	6+7,8	
5.....	..	..	7,8+9	8	8,9	8	8	8+9

The origin of the *ulnar* nerve was traced in thirty-two cases, of which fourteen were adult.

It was found to arise in four different ways. Its most common origin is from the 8th and 9th together. This occurred in twenty-three cases, eleven fœtal and twelve adult. With these is sometimes combined a strand from the 7th, as shown in five cases, four fœtal and one adult. In three fœtal cases it arose from the 8th only, and in one fœtal, and one adult case from the 7th and 8th. The 7th is only added to the nerve in some of those cases where it gives a branch to the inner cord. In several cases the branch from the 8th was much larger than that from the 9th. I have never seen the reverse.

The nerve was split down in seven instances, of which two were fœtal. One of these had fibres of the 7th in it which, however, were not traced separately. Branches were traced to the muscles of the forearm from both nerves in five cases, in two from the 8th alone. The anterior cutaneous branches were in four cases from the 9th, in one apparently from the 8th. The dorsal cutaneous was in all seven a branch of the 8th. The superficial division in the hand going to the fingers was traced in all, and in three contained fibres from both 8th and 9th, in the other four was wholly from the 9th, while the deep or muscular branch traced in six was in five wholly from the 8th, and in the sixth received also from the 9th.

According then to these dissections the 8th and 9th usually both supply the forearm muscles, the 8th gives the dorsal cutaneous, and serves the intrinsic muscles of the hand, while the 9th gives sensation to the skin on the palmar surface of the hand, and of the lower third of the forearm.

The origin of the *internal cutaneous* was noted twenty-three times in my dissections. Nine times it contained a fibre from the 8th as well as the 9th, fourteen times it sprang from the 9th alone. In two cases of the former class the 8th was separated from the 9th in the nerve. In the one the 9th alone supplied the skin of the arm, the 8th not entering the skin until below; in the other the 9th was seen to supply the skin of the arm and of the front of the forearm, while the 8th ran to the back of the forearm.

The *lesser internal cutaneous* is derived in all but very rare exceptions from the 9th alone. In twenty cases it only once received from the 8th also.

The posterior branches of the nerve roots unite to form the *posterior cord.* Each of the four upper roots contributes to the cord, but the 9th rarely joins it. Out of forty-five cases where the point is noted, a fibre from the 9th occurs only six times, three times in fœtal, three in adult cases. In one of these there was an unusually large root from the 10th nerve. In many adults the branch to the posterior cord can be seen to leave the 8th root before this is joined by the 9th. It may be taken then, as a rule, that the 9th does not contribute to the posterior cord. In the six exceptions the fibre from the 9th was invariably very small, and ran with the 8th, from which it was not isolated.

The branches of the posterior cord are the three subscapulars, the circumflex, and the musculo-spiral.

The *first subscapular*, which serves the subscapularis muscle, is often double, sometimes triple. It never receives from the 7th or 8th root, and is often wholly or partly given off from the 5th and 6th before they are joined by the 7th. Dissection shows that even when given off after the junction the 7th has still no share in it. This rule was invariable in forty-one cases. In twenty-three of these a dissection was made to see to which of the two upper roots the nerve was to be traced. It was found to come from the 5th in eleven cases, from the 6th in three, and from both in nine. I believe that owing to my not being sufficiently mindful that the muscle often receives more than one nerve, I have referred too large a number to the 5th alone. Whether this be so or no, it does not represent the whole supply given to it, for the lower part of the muscle always receives fibres from the *second subscapular* on its way to the teres major.

This is a nerve of rather lower origin than the preceding. It was traced three times to the 5th, all fœtal cases; thirteen times to the undivided 6th and 5th; four times to the 6th alone, one being an adult case; nine times to both 5th and 6th, seven being adult cases; nine times to both 6th and 7th, seven being adult cases; and thrice it was formed by a branch from the cord of the upper two roots, and one from the 7th. This shows out of forty-one cases twelve in which the 7th contributed to the nerve, and twenty-nine in which it was excluded. There were only three in which the supply lay above the 6th, and thirteen where it lay wholly below the 5th.

The question then arose—When the 7th enters the nerve, does it form part of the twigs which supply the subscapularis? I divided the nerve in three such cases, and found that in each the 7th went entirely to the teres major.

The lower part of the subscapularis, however, is served, though not

by the 7th, yet by a nerve which is of lower origin than the first subscapular. Thus in four instances, three of them adult, where the first subscapular came from the 5th alone, the second contained no 5th, and in these cases the lower part of the muscle was supplied by a lower nerve. This confirms also three other adult dissections of the first subscapular. In one it was triple; the first twig going to the upper fibres of the muscle ran from the 5th, the next to the middle fibres from the 5th and 6th, and the third from the 6th alone, below which came a twig from the second subscapular. In the two other cases the nerve was double, the upper twig coming from the 5th, the lower from the 6th root.

The *third subscapular* going to the latissimus dorsi was traced in forty-two cases. Once it came from the 5th and 6th alone, three times it was formed by the 7th and a branch from the undivided 5th and 6th, four times by the 7th and a branch from the 6th (once with an addition from the 8th also), twenty-one times from the 7th alone, and thirteen from both 7th and 8th.

It is to be noted that in these three muscles there seems a regular progression from above downwards. The latissimus dorsi is usually served by a lower nerve than the teres, and this by a lower than the subscapularis, and though they are occasionally equalised their positions in the series are never reversed.

The *circumflex* nerve was shown in forty-three cases to be derived from the 5th and 6th alone. It never received a fibre from the 7th. In six cases, two of them adult, it came from the 5th alone, in twenty-two from both 5th and 6th, and in the remainder it was undivided. It never arose from the 6th alone, and in many cases where both nerves helped to form it the branch from the 6th was so small that it could not be traced without breakage. I dissected the nerve three times. In all these the teres minor was supplied by the 5th only, and the deltoid by both 5th and 6th. The cutaneous branch was in one from the 5th alone, in the other two received a fibre from the 6th also.

The *musculo-spiral* is formed sometimes by all four of the upper roots, usually by the 6th, 7th, and 8th alone. Out of forty-six cases the 5th and 6th were undivided in twelve, in nine of the remaining thirty-four the 5th helped to form the nerve. Two of these were adult. In twenty-five, fourteen of which were adult, it was excluded.

In one adult case where the 5th entered the nerve it was found by dissection to run to the external cutaneous branch alone.

The branch first given off is the nerve to the long head of the triceps, with which sometimes goes the internal cutaneous branch. The former was twelve times given from the 8th root, thrice from both 7th and 8th, and once from the 7th alone. The internal

cutaneous was seven times noted separately, in all of which it came from the 8th alone. That part of the muscle which arises below the musculo-spiral groove, commonly called the inner head, receives the ulnar collateral nerve on its inner part, and another branch which also serves the anconeus in its outer part. These two nerves were in six instances derived from the 8th only, in three from both 7th and 8th, in two from the 7th only, and in one from the 7th with the addition of a fibre from the 6th. In eleven cases where the supply of the outer head of the triceps was traced separately, it came from the 8th alone once only, from both 7th and 8th twice, from the 7th alone twice, from the 6th and 7th once, and from the 6th alone in five cases. In fourteen instances the nerves to the separate heads were not traced apart, but the whole muscle together with the internal cutaneous branch was in all supplied below the 6th. In three more the inner and outer heads were taken together, and in these also the supply was from the 7th and 8th.

The 6th therefore only entered the triceps in five out of twenty-eight cases, and it is to be noted that in No. 19, the only case where the 6th entered the inner head, it wholly supplied the outer.

The next branches given off are the two external cutaneous. The short branch was noticed in fourteen cases. Eight times it was shown to come from the 6th alone, once from both 5th and 6th, and in the remaining five from the first two roots, the 5th not being excluded. The long branch sprang from the 6th five times, in two of which the 5th was not excluded, from both 6th and 7th once, from the 7th thrice, from the 7th and 8th four times,* and from the 8th thrice.

The nerve then supplies the brachialis anticus. The branch, or branches, going to this muscle, which are very small, were isolated eight times. They were traced each time to the 6th, but in three cases the 5th was not excluded.

The branch to the supinator longus was traced twenty-two times, always to the 6th, but in twelve cases the 5th was not excluded.

The extensor carpi radialis longior was served twelve times by the 6th alone (in eight the 5th was not excluded), once by both the 6th and the lower nerves, and ten times by the lower part of the musculo-spiral, in four of which the 7th alone was traced to it, in five the 7th and 8th were not separated, and in one both were found supplying it.

The brevior was supplied by the 6th six times, in four of which the 5th was not excluded, once by both the 6th and the lower nerves, and nine times by the lower nerves alone, in four of which the 7th was the agent, and in one both 7th and 8th.

* In one case formed by the 7th and 8th the twig from the 7th ran nearer the radial border than that from the 8th.

The nerve to the supinator brevis was traced thirteen times, in every case to the 6th, but in five the 5th was not excluded.

The two remaining divisions are the radial and the posterior interosseous. The former was composed by the 6th alone thirteen times, in seven of which the 5th was not excluded, twelve times it was partly formed by the lower nerves. In all the seven cases when the two lower nerves had been divided the 7th alone was found to share in the radial.

I was able in only one case to dissect the radial consisting of both 6th and 7th. In it the 6th supplied the ball and dorsum of the thumb and the radial side of the index, while the 7th took the remainder.

The posterior interosseous was in seven cases formed by the lower nerves entirely. In four it received also part of the upper nerves. The question arose whether the 8th ever entered the muscles of the forearm. Out of sixteen dissections, eleven fœtal and five adult, twelve of which four were adult showed the 8th to cease before the forearm muscles were reached, four of which two were adult showed the 8th coming round to the back of the arm.

In two of these latter the 7th and 8th formed equal parts of the posterior interosseous; in the other two cases the 8th gave so minute a fibre to the nerve below the triceps that I could not trace it separately.

In several of these cases the 5th was not excluded from the musculo-spiral, and must therefore be considered as a possible source of the supply, but it must be remembered that the probability of its taking part in the nerve is only, as before shown, nine in thirty-four, and that when dissected in the nerve it was found only to enter the short external cutaneous branch.

These results show considerable variation in the distribution of the nerve roots, although I do not think it is greater than in any other of the structures of the body. But this variation is not extravagant. If a type be composed from the foregoing materials and compared with the varieties, it appears that if a muscle or a piece of skin is not supplied by the typical nerve, the place is filled only by one of its neighbours, not by a nerve far removed from it in the series. The teres major usually supplied by the 6th is on occasion supplied by the 5th, and sometimes by the 7th, never by the 8th or 9th.

Again, some muscles seem to bear definite relations to each other, and their nerve supply seems also to vary solidly, so that the relative position of the muscles judged by their nerve supply does not alter although they be not served by the usual nerve. The best example of this is in the three muscles which are attached along the inner side of the bicipital groove, the subscapularis, teres major, and latissimus dorsi. The first is usually supplied by the 5th and 6th, the

second by the 6th, and the last by the 7th, and however much they vary above and below their typical place, they do not change their relations to each other. A similar relation exists between the two supinators and the two radial extensors. These last are sometimes supplied by the 6th, sometimes by the 7th, but they are never in any case placed above the supinators. These are always supplied by the 6th alone. The flexor group in the forearm show a similar fixed relation.

From consideration of the usual regularity of the nerve supply, and of the limits within which alone it varies, I conclude that the nerve roots are not always composed of the same fibres, but that what is in one case the lower bundle of the 5th may be in another the upper of the 6th, and what is now the upper bundle of the 8th will at another time be the lower of the 7th root. This may be expressed as a law.

LAW I.—Any given fibre may alter its position relative to the vertebral column, but will maintain its position relative to other fibres.

It is to confirm laws that exceptions are important. One of my infants showed on both sides a larger root from the 10th or 2nd dorsal nerve than was usual. On the right side the nerve was slightly bigger than the natural, but on the left it was as large as the 9th, and this as large as the 8th, whereas the natural proportion of the 8th to the 9th is about 2 to 1, and the 10th root is a minute fibre only. On the right side the only abnormality in the plexus was that the 9th sent a branch to the musculo-spiral. But on the left the musculo-cutaneous received from the 7th, the median received no 6th, the teres major was supplied by the 7th alone, the circumflex received from the 7th, and the musculo-spiral was formed by the 7th, 8th, and 9th. In the ulnar both 8th and 9th entered the deep branch in the hand. Nevertheless the 4th sent a communication to the 5th, and the suprascapular and subscapular were given off normally.

It appears that in this case the representation of the muscles in the spinal cord began at the ordinary level, but was more than usually lengthened out, its lowest point sinking so far that the 10th nerve conveyed an excessive proportion of fibres to the plexus. This unusual formation of the peripheral nerves still maintains the relative position of the muscles.

I do not, however, find that every muscle comprised in the table holds fixed relations with all others. There appear to be groups of muscles which are not intimately related. Thus the rise or fall of the three subscapular nerves does not entail a like movement in the nerves of the forearm.

I have drawn up from my dissections the following table, giving the usual supply of the muscles of the upper limb.

Usual nerve supply.	Muscles.
3rd, 4th, and 5th	Levator anguli scapulæ.
5th.	Rhomboids.
5th, or 5th and 6th.	Supra-spinatus. Infra-spinatus. Teres minor.
5th and 6th.	Subscapularis. Deltoid.
	Biceps. Brachialis anticus.
6th.	Teres major. Pronator teres. Flexor carpi radialis. Supinator longus and brevis. Superficial thenar muscles.
5th, 6th, and 7th.	Serratus magnus.
6th or 7th.	Extensores carpi radialis.
7th.	Coraco-brachialis.
	Latissimus dorsi.
	Extensors at back of forearm.
	Outer head of triceps.
7th and 8th.	Inner head of triceps.
7th, 8th, and 9th.	Flexor sublimis.
	Flexor profundus, carpi ulnaris, longus pollicis, and pronator quadratus.
8th.	Long head of triceps.
	Hypothenar muscles.
	Interossei. Deep thenar muscles.

The pectoralis major receives from the 6th, 7th, 8th, and 9th; the minor from the 7th, 8th, and 9th.

The question next arises upon what system are these muscles innervated? Is it a system of form or of function? Are muscles supplied by the same nerve because they act together or because they lie near one another?

In the first place the movements of the arm are so varied that there is hardly any combination of muscles unrepresented. In the commonest and most necessary of all acts, that of putting a piece of food into the mouth, the food is grasped by the small muscles of the thumb and the interossei, the carpus is flexed upon the forearm, the forearm is half pronated and flexed upon the arm, and the pectoralis major draws the limb forward across the chest. Doubtless many other muscles take an unrecognised share also. But of those here mentioned the anatomy of ordinary text-books shows that some, the flexors of the elbow, are served by the outer cord, the 5th, 6th, and 7th, whilst the interossei are supplied by the ulnar from the 8th and 9th. According to my dissections that one action is brought about through every nerve in the plexus, and there is no action which does not involve several nerves at once.

Looking at the theory from another point of view, is it true that muscles which cause the same movement, or which continually con-

tract together, are supplied by the same nerve? The action of the two pronators is indistinguishable, but the teres is supplied by the 6th, and the quadratus by the 8th and 9th; the thumb is always acting with the fingers, yet the superficial thenar muscles are served by the 6th, and the others by the 8th.

It seems then certain that the place where functions are represented is higher than the peripheral nerves, and that these are distributed according to some other plan.

The other system that suggests itself is the system according to place, and I find that the nerve supply of the muscles of the upper limb obeys three rules.

LAW II.—A. *Of two muscles, or of two parts of a muscle, that which is nearer the head-end of the body tends to be supplied by the higher, that which is nearer the tail-end by the lower nerve.*

B. *Of two muscles, that which is nearer the long axis of the body tends to be supplied by the higher, that which is nearer the periphery by the lower nerve.*

C. *Of two muscles, that which is nearer the surface tends to be supplied by the higher, that which is further from it by the lower nerve.*

The first rule has been already clearly exemplified in the supply of large flat muscles which receive more than one nerve, such as the pectoralis major and serratus magnus. The subscapularis is also an example. In all these muscles it was shown that the upper parts were supplied by upper, the lower by relatively lower nerves. It remains to show how it is exemplified in the remainder of the muscles belonging to the limb.

It has been laid down that in the first position of the fœtus the great tuberosity and the external condyle of the humerus, the radius and the thumb are turned towards the head; the lesser tuberosity, the internal condyle, the ulna, and the little finger towards the tail. The first set of points have been called pre-axial, the latter post-axial. The muscles corresponding with the pre-axial points will be found to be supplied by higher nerves than those connected with post-axial points at the same level of the limb.

Of the muscles which connect the scapula to the spine, the highest is the levator anguli scapulæ. This is supplied by the 3rd and 4th, and slightly by the 5th. Below this come the rhomboids supplied by the 5th alone. Of the muscles which join the humerus to the scapula those inserted into the outer or pre-axial tuberosity are supplied by the 5th, with very slight, if any, aid from the 6th. Of those going to the post-axial part, the subscapularis, which is the highest, is supplied by the 5th and 6th; the teres major below it is supplied by the 6th, the latissimus dorsi which comes from lower down the body, and the

coraco-brachialis continuing down the post-axial border, are served by the 7th.*

Of the muscles running to the upper end of the forearm, the biceps and brachialis anticus are supplied from the 5th and 6th; the triceps by the 7th and 8th, and it is remarkable that of the three heads of the triceps the outer, which is nearest to the pre-axial border, is the highest in the series.

At this point the 5th ends.

Of the muscles arising about the outer condyle, the highest in rank are the two supinators inserted into the radius, next the extensors of the carpus going to the radial side of the hand. Of those coming from the internal condyle, the two outermost, nearest, that is, to the radius, are supplied by the 6th. The innermost, the flexor carpi ulnaris, is supplied by the 8th and 9th (and at this point the 9th first begins to share in the system); the flexor sublimis, between the two, is supplied by the 7th and the lower nerves. None of the deep muscles are served by the 6th, and the 7th seldom runs to them. They are as a rule innervated by the lowest of the series.

Passing to the hand, the superficial muscles on the outer side are supplied by the 6th, the superficial on the inner side by the 8th, the deep muscles being again supplied by the lowest of the nerves supplying the hand, namely, the 8th.

It is remarkable and unexpected that the 8th and not the 9th should be the nerve going to the muscles of the hand. The explanation I would suggest is that the motor part of the 9th is rather an auxiliary nerve than an integral part of the plexus. The extremely small part which it often plays in the ulnar, and the fact that in two cases none of its fibres could be traced thither at all, confirm this view.

The cutaneous supply of the limb exemplifies still more clearly the first of the rules laid down for the motor system. Over the deltoid runs the descending branch from the 3rd and 4th, below this comes the circumflex from the 5th, or from the 5th and 6th. The branch of the 5th in the musculo-cutaneous, and the short external cutaneous of the musculo-spiral given by the 6th, supply the skin over the outer condyle and head of the radius. Down to the end of the radius it is served by the musculo-cutaneous, either from the 6th alone, or as in one case by the 5th and 6th; the thumb is supplied by the 6th in the median and radial.

On the inner side, the highest part, the skin of the axilla, is served by the 10th, the area below this by the junction of the 9th and 10th (the intercosto-humeral with the nerve of Wrisberg), next by the 9th

* The deltoid, which would also exemplify the law, is omitted. I consider it uniform with the pectoralis major, not truly part of the long muscles of the limb.

in the internal cutaneous which runs down to the wrist. The little finger is supplied by the 9th in front, and by the 8th behind; the front of the three middle fingers by the 7th and 8th, the back by the 6th and 7th.

In the sensory system, therefore, as in the motor, the upper nerves can be traced down the pre-axial, and the lowest down the post-axial border; but a remarkable difference appears between the sensory and the motor system. Whereas in the latter it seemed the tendency for the lowest nerves to supply the lowest muscles, in the sensory nerves the extreme are nearest the upper, the middle nerves nearest the lower extremity of the limb.

Thus, if the limb be seen from the front the two highest nerves on the outer and inner sides respectively are the 4th and the 10th. Lower than these the 5th and 6th take the outer, the 9th and 10th the inner side. Below the elbow the 6th alone takes the outer, and the 9th alone the inner. In the hand, while the 6th and 9th continue their positions, the 7th and 8th for the first time begin to join in the supply.

Below the elbow a distinction begins to appear between the dorsal and palmar surfaces. If a line be drawn round the lower third of the forearm from the middle of the front surface to the radial border, across the dorsum, round the ulna, and so to the middle line again, it will cut in order cutaneous branches of the 6th, 7th, 8th, and 9th. Thus on the outer side behind the 6th in the musculo-cutaneous comes the musculo-spiral cutaneous from the 7th, and on the inner side behind the 9th in the internal cutaneous comes the dorsal cutaneous formed by the 8th. When the 8th was dissected in the internal cutaneous it was found going to the dorsum, and where the 7th and 8th joined to form the musculo-spiral branch the 7th lay nearest of the two to the radial border.

In the fingers, again, the 8th is the lowest nerve supplying the back. The 9th the lowest in front. The extremes therefore are on the palmar, the dorsal area occupies an intermediate position. Above the elbow the 8th in the internal cutaneous of the musculo-spiral, holding a position intermediate between the 10th in the intercosto-humeral, and the 5th and 6th in the circumflex, is an example of the same law.

The sensory nerves therefore obey the following rules:—

A. *Of two spots on the skin that which is nearer the pre-axial border tends to be supplied by the higher nerve.*

B. *Of two spots in the pre-axial area the lower tends to be supplied by the lower nerve, and of two spots in the post-axial area the lower tends to be supplied by the higher nerve.*

This, however unexpected, is not very difficult to understand. The

epiblastic layer which forms the skin ensheaths the mesoblast, from which the deeper structures are developed. Suppose nerves, or what will afterwards become nerves, to be distributed to both. The mesoblast now begins to grow and pushes before it the enveloping sheath of epiblast. But, as may be seen with a piece of india-rubber, in any such process the points furthest from the centre of pressure remain nearest the top of the tube, and the point which was in the middle when the stretching began, will be at the furthest point of the sheath when it is finished. If in the arm the centre of pressure be supposed opposite to the area supplied by the 7th nerve, this nerve will always tend to supply the parts lying nearest the axis of the limb, and furthest from the axis of the body, while the 6th, 5th, and 4th in the pre-axial, and the 8th, 9th, and 10th in the post-axial area, will in that order approach the trunk. This is the case.

It appears then that in both sensory and motor systems the pre-axial area is supplied by higher nerves, the post-axial by lower; that the supply of the skin follows rules which obtain in any membrane subject to the same conditions, but that the supply of the muscles is modified by laws peculiar to themselves.

This subject has not to my knowledge been before this investigated by human dissections.

Ferrier's* classification of the muscles by means of electric stimulation of the spinal roots in monkeys, though he explains the system as one of function, is not far removed from that now put forward.

Forgue,† who stimulated not the spinal roots, but the branches which they gave to the nerves of the plexus, and who watched the contraction of dissected muscles, draws up the following list for monkeys:—

Median.‡

Médian externe.	5 and 6	Biceps. Brachialis anticus. Clavicular part of deltoid. Pronator teres. Flexor carpi radialis.
	7	Biceps slightly. Pronator teres. Flexor carpi radialis.
Médian interne.	8 and 9	Pronator teres and flexor carpi radialis slightly. Flexor sublimis. Flexor profundus. Flexor carpi ulnaris. Intrinsic muscles of hand.

* 'Roy. Soc. Proc.,' vol. 32, 1881, p. 12.

† 'Distribution des Racines Motrices dans les Muscles des Membres.' Émile Forgue. Montpellier, 1883. Page 45.

‡ Forgue's median is both median and musculo-cutaneous. Médian externe is the musculo-cutaneous and the outer head of the median, médian interne the inner head.

*Radial.**

5 and 6 { Deltoid from scapula. Supinator longus. Extensores carpi radiales.

7 { Latissimus dorsi. Teres major. Triceps. Extensores carpi radiales. Extensors slightly.

8 and 9 .. Latissimus dorsi. Triceps. Extensors.

Cubital.†

Flexor profundus. Flexor carpi ulnaris. Intrinsic muscles of hand.

He lays down also the following laws:—‡

1. Each root furnishes branches to two systems, an anterior and a posterior.§
2. As the stimulus approaches the dorsal pairs, the contraction occurs in lower segments of the limb.
3. As the stimulus approaches the dorsal pairs, the contractions pass from the radial to the ulnar border.

He also adds—

"It is a secondary law that the superficial layers are supplied before the deep."

Both these observers worked with monkeys, and Forgue's laws are, with the exception of the first, identical with those which human dissections have produced for me. That the details should exactly correspond is not to be expected in two different genera when individuals of the same vary so widely.

Electrical stimulation does not show the sensory supply.

I have often tried to complete this account by dissecting the nerves upwards to the spinal cord. I have, however, never been able to rely on the results. The connective tissue permeating the nerve separates and protects the bundles of nerve fibres composing it, and renders their dissociation impossible. But as the nerve nears the intervertebral foramen this tissue very rapidly diminishes, and in the foramen the root consists of nerve bundles with hardly any connective tissue between them. The nerve bundles in the adult might perhaps be separated even here from one another, but in the fœtus, and these alone are for this purpose accessible to me, their minuteness and their softness have prevented any satisfactory dissection.

* The radial here means the posterior cord of the plexus.

† Ulnar.

‡ Pp. 41–43.

§ This refers to the adult position. A truer view is to take the earliest observed position in the fœtus.

December 9, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

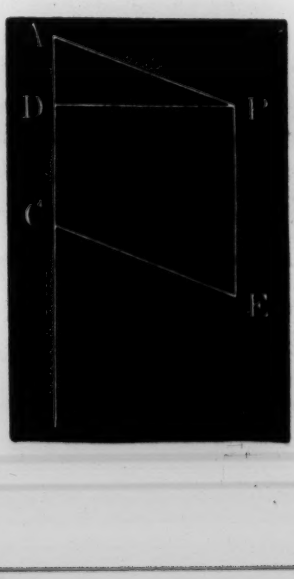
The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Dr. Archibald Geikie.
Professor Bartholomew Price.
Sir George Richards.

The following Papers were read:—

I. "Note to a Paper on the Geometrical Construction of the Cell of the Honey Bee ('Roy. Soc. Proc.,' vol. 39, page 253)." By HENRY HENNESSY, F.R.S., Professor of Applied Mathematics in the Royal College of Science, Dublin. Received November 16, 1886.

The result obtained in the paper on the cell of the honey bee, read November 26, 1885, by which the side of one of the lozenges composing the cell was found to be three times the difference between the two parallel edges forming the sides of one of the trapeziums of the prism, gives a very simple method for constructing the figure as follows. On a straight line take a part AD, and lay



off DC equal to twice AD, from D erect a perpendicular, and with radius AC = 3DA cut off DP; AC and AP are sides of the lozenge ACEP, which fulfils the required conditions. It is manifest that from this lozenge the remaining two lozenges and also the six trapeziums can be immediately constructed.

The triangular pyramid which terminates the bee's cell may be inscribed in a sphere whose diameter is three times the side of one of the edges of the pyramid. The base of this pyramid is an equilateral triangle, the side of which is $h\sqrt{3}$, and whose circumscribing circle has $2h$ for its diameter. This diameter is a chord of the spherical segment whose versed sine is x. Hence, if D is the diameter of the sphere in which $2h$ is a chord, $x\mathrm{D}=h^2+x^2$, but also $h=2\sqrt{2}x$, and $s=3x$, whence

$$\mathrm{D}=9x=3s.$$

We have also

$$\mathrm{D}=\frac{9h}{2\sqrt{2}}>2h.$$

Hence the sphere contains within it all that part of the bee's cell bounded by the three lozenges, together with as much of the hexagonal prism as may be measured by twice the side of a lozenge on the shorter edge of the prism.

This result, together with the extremely simple mode now given for constructing the figure, divests the problem of the complexity and difficulty with which it was formerly sometimes regarded, and it may also possibly enable the naturalist to more readily explain the action of the bees in moulding the cells of the honeycomb to their observed shapes.

II. "A New Method for the Quantitative Estimation of the Micro-organisms present in the Atmosphere." By PERCY F. FRANKLAND, Ph.D., B.Sc., F.I.C., F.C.S., Assoc. Roy. Sch. Mines. Communicated by Professor FRANKLAND, D.C.L., F.R.S. Received November 15, 1886.

(Abstract.)

The author commences by giving a sketch of some of the more important methods which have been devised for the bacterioscopic examination of air. In these he includes the experiments of Pasteur, who was the first to show that the air at different places varied in the number of micro-organisms which it contained, and of Tyndall, who proved that the microbes suspended in the air become rapidly deposited in the absence of any disturbing influence. He further

describes the process of Freudenreich and Miquel for the quantitative estimation of the bacteria in air, and points out the great advance which has been made upon their method in the adaptation by Koch, and later by Hesse, of a solid nourishing medium for their investigation. In reviewing these different processes he draws attention to the advantages and disadvantages attending them, and proceeds to describe a new method which he has devised, and in which he has endeavoured to overcome some of the objections to which the others are open.

The first experiments consisted in aspirating a definite volume of air through plugs of either glass-wool or sugared glass-wool, and mixing them by violent agitation with a definite volume of either broth or sterilised distilled water.

A portion of this liquid was then added to gelatine-peptone, and plates were poured in the usual manner.

In this way a much larger volume of air was capable of being examined than was possible by Hesse's method, whilst at the same time the solid medium, with its advantages, was retained. The experiments, however, show that although in many cases the results of two or more plates poured from the same mixture were fairly uniform, yet discrepancies did occur, and were sometimes very considerable, pointing to the fact that the organisms had not become evenly distributed throughout the liquid; also as only a portion of the air aspirated was capable of being examined, a very much larger volume of air had to be used in order to give a sensible result.

This objection, which applies equally to Miquel's method, which rests upon the assumption that it *is* possible to equally apportion out into a series of flasks or tubes the organisms contained in such a liquid, led the author to abandon the plate process, and to devise a method which should enable the whole volume of air aspirated to be examined.

The method consists essentially in aspirating a known volume of air through a glass tube, containing two sterile plugs, consisting either of glass-wool alone, glass-wool and fine glass-powder, glass-wool coated with sugar, or sugared glass-wool and fine sugar-powder. The plugs are so arranged that the first one through which air is drawn is more pervious than the second. After a given volume of air has been aspirated, the two plugs are transferred respectively to two flasks, each containing melted sterile gelatine-peptone, and plugged with sterile cotton-wool stoppers. The plug is carefully agitated with the gelatine, so as to avoid any formation of froth, and when the plug has become completely disintegrated and mixed with the gelatine, the latter is congealed, so as to form an even film over the inner surface of the flask. On incubating these flasks at a tempe-

rature of 22° C., in the course of four to five days the colonies derived from the organisms contained in the plugs make their appearance, and can be readily counted and further examined.

A large number of experiments were made with a view of testing the accuracy of the process. For this purpose experiments were conducted, using sometimes single plugs, and sometimes double, and it was almost invariably found that all the organisms were deposited on the first plug, the second plug in the very exceptional cases when it did yield anything, containing rarely more than one organism.

In connexion with Hesse's method, it was found that in experiments performed in the open air, when a blank Hesse tube was exposed side by side with the one through which air was being aspirated, a number of organisms also became deposited in the blank tube, thus introducing an important source of error in the quantitative results obtained by Hesse's process. In the flask method, on the contrary, such blank tubes rarely contained any organisms, and in no case more than a vanishing proportion of those present in the other tube. This shows that whereas in Hesse's apparatus any disturbance of the air during the experiment vitiates the accuracy of the result, in the flask method such disturbances are immaterial.

On the other hand, in the absence of aërial currents, there was a remarkable concordance between the results obtained by Hesse's method and by the "flask method." This is important, not only as showing the quantitative accuracy of the new method, but by clearly demonstrating that the organisms present in the air exist in an isolated condition, and not in aggregates, as suggested by Hesse. It will be remembered that the plug is violently agitated with the gelatine-peptone, during which operation such aggregates would undoubtedly be broken up wholly, or, any rate, partially; it would, therefore, be reasonable to expect that the "flask method" would yield a larger number, and possibly a far larger number of colonies than those formed in Hesse's tubes, but as, on the contrary, the numbers agree, under the circumstances described, in so remarkable a manner, it points to the fact that they exist in an isolated condition.

The paper is illustrated with drawings and photographs.

The following are the principal advantages which the author claims for the "flask method."

1. The process possesses all the well-known advantages attaching to the use of a solid medium.

2. The results, as tested by the comparison of parallel experiments, can lay claim to a high degree of quantitative accuracy.

3. The results, as tested by control experiments, are not appreciably affected by aërial currents, which prove such a disturbing factor in the results obtained by some other methods.

4. The collection of an adequate sample of air occupies a very

short space of time, so that a much larger volume of air can be conveniently operated upon than is the case with Hesse's method. Thus whilst the aspiration of 10 litres of air through Hesse's apparatus takes about three-quarters of an hour, by the new method about 48 litres can be drawn through the tube in the same time, whilst a better plan is to take two tubes and alternately draw a definite volume of air through each, as by this means duplicate results are obtained.

5. As the whole plug, upon which the organisms from a given volume of air are deposited, is submitted to cultivation without subdivision, no error is introduced through the multiplication of results obtained from aliquot parts, and all the great difficulties attending equal subdivision are avoided.

6. The risk of aërial contamination in the process of *flask-cultivation* is practically *nil*.

7. The apparatus required being very simple and highly portable, the method is admirably adapted for the performance of experiments at a distance from home, and in the absence of special laboratory appliances.

III. "Further Experiments on the Distribution of Micro-organisms in Air (by Hesse's method)." By PERCY F. FRANKLAND, Ph.D., B.Sc., F.I.C., F.C.S., and T. G. HART, A.R.S.M. Communicated by Professor FRANKLAND, D.C.L., F.R.S. Received November 22, 1886.

(Abstract.)

The authors record a number of experiments, made with Hesse's apparatus, on the prevalence of micro-organisms in the atmosphere. The results are intended to form a supplement to those already obtained by one of the authors, and published in the last volume of the Society's 'Proceedings' (vol. 40, p. 509). The greater number of the experiments have been performed on the roof of the Science Schools, South Kensington, the air of which has now been under observation at frequent intervals during the present year. The authors point out the variations according to season, which have taken place in the number of micro-organisms present in the air collected in the above place. The average results obtained were as follows:—

1886.	Average number of micro-organisms found in 10 litres of air by Hesse's method.
January	4
March	26
May	31
June	54
July	63
August	105
September	43
October	35

Experiments are also recorded showing the enormous increase in the number of micro-organisms present in the air of rooms consequent on crowding. In illustration of this point the authors cite a series of experiments made in the Library of the Royal Society during the evening of the conversazione in June last, on which occasion the following results were obtained:—

Royal Society's Library.	Number of micro-organisms found in 10 litres of air.
June 9, 1886.	
9.20 p.m.	326
10.5 „	432
June 10, 1886.	
10.15 a.m.	130

In addition to determining the number of organisms present in a given volume of air, the authors have also, in each case, roughly estimated the number falling on a given horizontal surface by exposing dishes filled with nutrient gelatine and of known superficial area, as in the experiments previously published.

IV. "On the Intra-ovarian Egg of some Osseous Fishes." By ROBERT SCHARFF, Ph.D., B.Sc. Communicated by Professor MCINTOSH, F.R.S. Received November 17, 1886.

(Abstract.)

These researches were carried out while acting as assistant to Professor McIntosh, at the St. Andrew's Marine Laboratory. The chief material consisted of the intra-ovarian ovum of the gurnard (*Trigla gurnardus*). Many other marine forms, however, were examined.

The paper has been divided into the following five paragraphs:—

I. *The Nucleus and its Changes in the Smaller Ova.*

In the smallest ova the nucleus occupies almost the whole of the interior, and the nucleoli are mostly attached to the inner surface of the nuclear wall. In somewhat larger eggs, the protoplasm surrounding the nucleus has increased, and is seen to be divided into a darker internal portion and a lighter external one. The ring of dark protoplasm becomes separated off from the nucleus in the later stages, and ultimately disappears. The dark protoplasm has no doubt originated from the nucleus. The view that this has been caused by a substance being added from the nucleus is considerably strengthened by an observation made by Ransom, and published in the 'Philosophical Transactions,' 1867. He found in fact that the germinal spots were soluble in some of the constituents of the yolk. At this stage the spots become vacuolate, and assume a variety of different forms, until the nucleus enters a new phase in the development of the intra-ovarian egg described in the following paragraph.

II. *The Larger Ova and the Formation of the Yolk Spherules.*

The egg has almost reached its final size, although far from being mature, when the nucleus is seen to have shrunk a little, and from it project protuberances on all sides. These protuberances or diverticula, most of which contain nucleolar particles, are ultimately constricted off from the nucleus, and travel towards the periphery of the egg. A similar transformation of the nuclear contents has recently been observed by Balbiani, Roule, and Fol, in invertebrate ova, and by Will, in amphibia. The buds with their enclosed contents form the yolk spherules, the solid mass in their interior soon breaking up into fine granules. Both Gegenbaur and Balfour speak in support of the view that yolk spherules originate within the egg. As the egg reaches maturity the nucleus degenerates still more, but I believe it never entirely disappears.

III. *The Egg Membranes.*

Much has been written on this subject, and it is still doubtful how many membranes exist. Almost all observers, however, agree that in the mature egg there is a membrane pierced by minute pores, which has generally been called "zona radiata," though other terms, such as vitelline membrane, egg-capsule, &c., have been applied to it. In the intra-ovarian egg of the gurnard I found a semi-fluid layer inside the zona, corresponding to the "helle Randschicht" described by Gegenbaur in the ova of birds and reptiles. It disappears in the ripe ovum. No membrane external to the zona, such as mentioned by various observers, was seen in this or other fish eggs.

With regard to the pores in the zona radiata, it seems very probable

that they are filled with processes from the follicular epithelium, and that the egg is nourished in this manner. In the ova I examined I could not see any processes, but they have been noticed by other observers in larger eggs of fishes, as well as of reptiles and mammals.

IV. *The Follicular Layer.*

The follicular layer in the mature ovum consists usually of a layer of closely-set cells, which, seen from above, have an hexagonal appearance. A peculiar modification of the follicular cells is found in the shanny's egg (*Blennius pholis*). On one-half of the egg's surface the cells are elongated, their depth gradually increasing towards a central point. In this way the depth of the cells varies from 0·007 mm. to 0·032 mm. I never noticed follicular cells passing through the zona radiata, as has been described by many authors.

V. *Development.*

No observation was made as to the origin of the egg, and it could not be determined whether the ovum originated from a simple transformation of an epithelial cell, or whether several unite, as in "elasmobranchs." I am inclined to the belief, however, that Brock's and Kolessnikow's views are correct, according to whom only one cell is concerned in the formation of the primitive egg. In small ova the follicular epithelium is composed of a few large cells. There are several possible ways in which the follicular layer might have originated, either by an aggregation of epithelial cells round the ovum, as in elasmobranchs, or by a collection of connective tissue cells at the periphery of the ovum, or from the nucleus, as in many invertebrates. I have not been able to come to any definite conclusion on this subject.

The egg membranes appear after the follicle. The zona radiata is formed first, and, as well as the zonoid layer, it takes its origin from the yolk.

V. "Note on a New Form of Direct Vision Spectroscope." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor of Natural Philosophy, University of Cambridge. Received November 18, 1886.

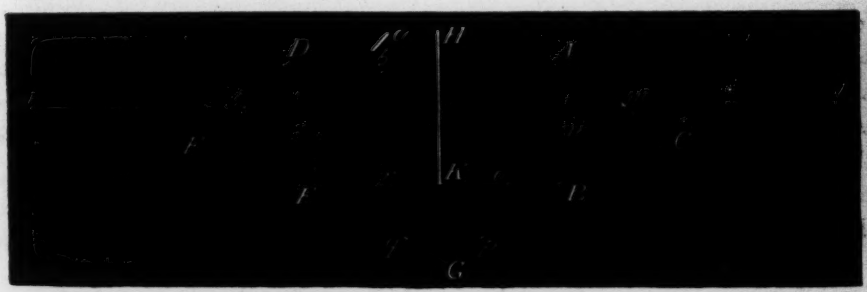
Direct vision spectroscopes are very useful in the observation of shifting objects, such as auroræ and other meteors. They are generally in request for telescopic work, and also in all cases where rapidity of observation is of consequence. Ordinary direct vision

spectroscopes with compound prisms have the disadvantage that the dispersion of the red end of the spectrum is small; less in proportion to that of the blue end than in spectroscopes with simple prisms. Also no measurements of lines can be made with them, except by means of a scale fixed in the field of view, which it is often difficult to see for want of illumination.

Some time since ('Roy. Soc. Proc.,' vol. 28, p. 482) we brought under the notice of the Society a direct vision spectroscope on Thollon's plan, which had not the faults of the instruments with compound prisms. It gave a dispersion equal to that of two prisms of 60°, and excellent definition, but the number of reflecting and refracting surfaces which had to be truly wrought was rather large, and the movement of the prisms by a screw made measurements with it slow.

Since then we have tried a spectroscope with one of the Astronomer Royal's half prisms, but we found it impossible to get good definition with the half prism for more than a small part of the visible spectrum; and in consequence faint bands near either end of the spectrum were quite invisible with this instrument.

The arrangement we have now to describe was intended to obviate the defects of the others.



It has three prisms symmetrically arranged, the middle one serving both for refraction and reflexion. The course of a ray through the prisms is indicated in the annexed diagram. A ray *lm* in the line of the axis of the collimator meets the first prism ABC in *m*, is refracted at *m* and *n*, meets the second prism EGB at *o*, and is then refracted, undergoes two internal reflexions at *p* and *q*, and is refracted out at *r*; it is then refracted through the third prism DEF at *s* and *t*, and emerges in the direction *tu*, which is a prolongation of its original direction *lm* and coincides with the axis of the observing telescope. The prism EGB is fixed, the other two prisms are movable about axes parallel to their edges passing through the points *m* and *t*. They are rotated simultaneously in opposite directions by a pair

of linked levers, of which one carries a graduated arc of $9\frac{1}{2}$ inches radius, by which the angle of rotation can be determined. Those rays which suffer no deviation in passing through the train, must all follow a course through the fixed prism parallel to *opqr*, whatever their refrangibility; but the angle of incidence at *o* will be different for the different values of the refractive index. By turning the two movable prisms into the positions shown by dotted lines the angle of incidence at *o* will be diminished, and a less refrangible ray will follow the course of no deviation. If then the first position of the prisms ABC, DFE, be that for which an extreme violet ray incident in the line *lm* suffers no deviation, all the less refrangible rays incident in the same direction may be successively brought to suffer no deviation by turning the prisms towards the position shown by the dotted line. The angles of the prisms have, of course, to be adjusted so that the extreme violet ray may suffer no deviation. A simple calculation suffices for this when the refractive indices of the glass employed are known. We have had the fixed prism constructed with the acute angles about $33\frac{1}{4}°$, and the movable prisms with angles about 62°. With these angles, when the movable prisms are so placed that the angles of incidence at *m* and of emergence at *o* are equal (the position of minimum deviation), the ray which suffers no deviation is one somewhat more refrangible than K of the solar spectrum. When then the prisms are turned, less refrangible rays are successively brought into the field of view, but no ray much more refrangible than K can be brought into the field. By increasing the acute angles of the fixed prism, or by diminishing the angles of the movable prisms, a longer range can be given to the instrument, but at the expense of some dispersion.

To prevent light passing directly from the collimator to the observing telescope, a stop HK is placed midway between the movable prisms.

It will be observed that the fixed prism serves both as a reflector and refractor, the dispersion produced by it being the same as that of a simple refracting prism of 46° (or 186°−4EBG) in the position of minimum deviation. The dispersion for the extreme violet is therefore that of two prisms of 62° and one of 46° in the position of minimum deviation. For less refrangible rays the position of the movable prisms is not that of minimum deviation and the dispersion is proportionally increased, so as to help, in a small degree, to correct the inequality of dispersion of the two ends of the spectrum. At the same time the symmetry of the arrangement is maintained for all rays when in mid-field, and sharp definition is secured for all parts of the spectrum. This is a most important character. We find that with our instrument A and H of the solar spectrum are equally well seen, and so are the red and violet lines of the flame spectrum of potassium.

For greater convenience in manipulation, and at the same time to ensure a large angular aperture, our instrument has a short collimator and telescope. The magnification of the image is in consequence but small, but nevertheless it is easy to see the nickel line between the two D lines in the solar spectrum, and the large angular aperture makes it easier to see faint spectra from large objects, as well as to catch the light of a moving object. With an eyepiece of low power it serves well for the observation of absorption spectra.

Measurements are made with it by bringing the line of which the position is to be measured to a fixed pointer in the middle of the field of view. The whole angle through which the prisms have to be moved in passing from A to H is 10° 15′, and as it is easy to read quarter minutes with the vernier, considerable accuracy of measurement may be attained. It must be observed, however, that the change of angle is not proportional to the change of refrangibility of the rays brought to mid-field, because a larger proportional rotation is required at the violet end, when the prisms are near the position of minimum deviation, than at the red end. Still there is a rotation of 33′ 30″ in passing from A to B.

The prisms are enclosed in a box, and though this is an advantage in viewing faint spectra, it would sometimes be difficult to see the pointer without some means of illuminating either the pointer or the field. We effect the latter object by a slit in the side of the box, and a small white paper reflector, *ab* in the figure, which throws light from the opening in the box on to the side of the object glass, clear of the prism. The slit in the box may be closed with a shutter or with glasses of different colours.

Lastly, the instrument can be used either with or without a stand. Without the stand it is light enough to be held in the hand and directed to the sky or to a moving object.

It has been constructed for us by Mr. Hilger with his usual skill.

December 16, 1886.

Professor STOKES, D.C.L., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Note on Specific Inductive Capacity." By JOHN HOPKINSON, M.A., D.Sc., F.R.S. Received November 9, 1886.

Consider a condenser formed of two parallel plates at distance x from each other, their area A being so great, or the distance x so small, that the whole of the lines of force may be considered to be uniformly distributed perpendicular to the plates. The space between the plates is occupied by air, or by any insulating fluid. Let e be the charge of the condenser and V the difference of potential between the plates. If the dielectric be air, there is every reason to believe that $V \propto e$, that is, there is for air a constant of specific inductive capacity. My own experiments ([1880] 'Phil. Trans,' vol. 172, p. 355) show that in the case of flint-glass the ratio of V to e is sensibly constant over a range of values of V from 200 volts per cm. to 50,000 volts per cm. From experiments in which the dielectric is one or other of a number of fluids and values of V upwards of 30,000 volts per cm. are used, Professor Quincke concludes ('Wiedemann, Annalen,' vol. 28, 1886, p. 549) that the value of e/V is somewhat less for great electric forces than for small. From the experiments described in that paper, and from his previous experiments ('Wiedemann, Annalen,' vol. 19, 1883, p. 705, *et seq.*) he also concludes that the specific inductive capacity determined from the mechanical force resisting separation of the plates is 10 per cent. to 50 cent. greater than that determined by the actual charge of the condenser. The purpose of the present note is to examine the relations of these important conclusions, making as few assumptions as possible.

The potential difference V is a function of the charge e and distance x, and if the dielectric be given of nothing else. The work done in charging the condenser with charge e is $\int_0^e V de$. If the distance of the plates be changed to $x+dx$, the work done in giving

the same charge is $\int_0^e \left(V+\frac{dV}{dx}dx\right)de$, hence the mechanical force resisting separation of the plates is $\int_0^e \frac{dV}{dx}de$. If the dielectric be air, $A\frac{V}{x}=4\pi e$, and the attractive force between the plates is $\frac{2\pi e^2}{A}$ or $\frac{AV^2}{8\pi x^2}$. If K_p be the dielectric constant as determined by an experiment on the force between the plates when the potential difference is V and distance is x,

$$K_p=\int_0^e \frac{dV}{dx}de \Big/ \frac{AV^2}{8\pi x^2}. \quad \ldots\ldots\ldots \quad (1)$$

If K be the dielectric constant obtained by direct comparisons of charge and potential,

$$K=\frac{4\pi xe}{AV}, \quad \ldots\ldots\ldots \quad (2)$$

whence

$$K_p\,K=\int_0^e \frac{dV}{dx}de \Big/ \frac{Ve}{2x}. \quad \ldots\ldots\ldots \quad (3)$$

We ordinarily assume that $V \propto xe$; if so, $K_p/K=1$. These results follow quite independently of any suppositions about the nature of electricity, about action at a distance, or tensions and pressures in the dielectric.

Yet another method of determining the dielectric capacity of fluids has been used by Professor Quincke. Let a bubble of air be introduced between the two plates, let the area of the bubble be A_1, and let P be the excess of pressure in the bubble above that in the external air when the potential is V, allowance being first made for capillary action.

The condenser now consists of two parts, one a fluid condenser area $A-A_1$, the other an air condenser area A_1, we have mechanical work done in increasing the area of the bubble from A_1 to A_1+dA_1, with constant charge—

$$e=\int_0^e \frac{dV}{dA_1}dA_1de,$$

but this work is $xPdA_1$,

whence

$$xP=\int_0^e \frac{dV}{dA_1}de.$$

Now

$$4\pi e=A_1\frac{V}{x}+(A-A_1)f(V),$$

where $4\pi e = \mathrm{A}f(\mathrm{V})$, when the whole space is occupied by fluid, and the distance is x.

The charge being constant we have—

$$0 = \left\{\frac{\mathrm{V}}{x} - f(\mathrm{V})\right\} d\mathrm{A}_1 + \left\{\frac{\mathrm{A}_1}{x} + (\mathrm{A} - \mathrm{A}_1) f'(\mathrm{V})\right\} d\mathrm{V},$$

and for the purpose of transforming the integral

$$4\pi de = \left\{\frac{\mathrm{A}_1}{x} + (\mathrm{A} - \mathrm{A}_1) f'(\mathrm{V})\right\} d\mathrm{V},$$

$$4\pi \frac{d\mathrm{V}}{d\mathrm{A}_1} de = \left\{f(\mathrm{V}) - \frac{\mathrm{V}}{x}\right\} d\mathrm{V},$$

whence

$$x\mathrm{P} = \frac{1}{4\pi} \int_0^{\mathrm{V}} \left\{f(\mathrm{V}) - \frac{\mathrm{V}}{x}\right\} d\mathrm{V}, \quad \ldots\ldots\ldots \quad (4)$$

$$x \frac{d\mathrm{P}}{d\mathrm{V}} = \frac{1}{4\pi} \left\{f(\mathrm{V}) - \frac{\mathrm{V}}{x}\right\}. \quad \ldots\ldots\ldots \quad (5)$$

Writing with Quincke K_s for the dielectric constant determined by a measurement of P, we have by substituting in (4) $f(\mathrm{V}) = \frac{\mathrm{K}_s \mathrm{V}}{x}$, and integrating as though K_s were constant,

$$x\mathrm{P} = \frac{1}{4\pi} \cdot \frac{\mathrm{K}_s - 1}{x} \cdot \frac{\mathrm{V}^2}{2},$$

$$\mathrm{K}_s = 1 + \frac{8\pi x^2 \mathrm{P}}{\mathrm{V}^2}, \quad \ldots\ldots\ldots \quad (6)$$

which may be taken as the definition of K_s,

whence

$$\mathrm{K}_s = \frac{2x^2}{\mathrm{V}^2} \int_0^{\mathrm{V}} \frac{f(\mathrm{V})}{x} d\mathrm{V}, \quad \ldots\ldots\ldots \quad (7)$$

but from (5) we have, since in fact $\mathrm{K} = \frac{x f(\mathrm{V})}{\mathrm{V}}$,

$$x \frac{d\mathrm{P}}{d\mathrm{V}} = \frac{1}{4\pi} \{\mathrm{K} - 1\} \frac{\mathrm{V}^2}{x}. \quad \ldots\ldots\ldots \quad (8)$$

But

$$\mathrm{K}_p = \int_0^e \frac{d\mathrm{V}}{dx} de \Big/ \frac{\mathrm{A}\mathrm{V}^2}{8\pi x^2}$$

$$= \int_0^{\mathrm{V}} \frac{d\mathrm{V}}{dx} f'(\mathrm{V}) d\mathrm{V} \Big/ \frac{\mathrm{V}^2}{2x^2}$$

$$= \frac{2x^2}{\mathrm{V}^2} \left\{f(\mathrm{V}) \frac{d\mathrm{V}}{dx} - \int_0^{\mathrm{V}} f(\mathrm{V}) \frac{d}{d\mathrm{V}} \frac{d\mathrm{V}}{dx} d\mathrm{V}\right\}.$$

Hitherto we have made no assumption excepting that energy is not dissipated in a condenser by charge and discharge. We now make an assumption concerning $f(V)$, namely, that it is of the form $\phi(V/x)$, *i.e.*, that $\frac{dV}{dx}=\frac{V}{x}$, or in words, that the capacity of a condenser varies inversely as the distance between the plates.

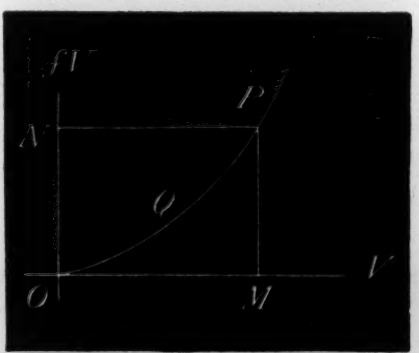
Then we have—

$$K_p = \frac{2x^2}{V^2}\left\{f(V)\frac{V}{x}-\int_0^V \frac{f(V)}{x}dV\right\}$$

$$= 2K-K_s. \quad \ldots\ldots\ldots\ldots\ldots \quad (9)$$

In words, the specific inductive capacity as determined by charge or discharge of a condenser at any given potential and distance between the plates is the arithmetic mean of the inductive capacity determined by the force resisting separation of the plates and of that determined by lateral pressure, the potential and distance being the same. This is true whatever be the relation between charge and potential difference, but it is at variance with the experimental result that K_p and K_s are both greater than K.

Further $$\frac{K_p}{K} = \int_0^V Vf'(V)dV \Big/ \tfrac{1}{2}Vf(V).$$



In the accompanying curve, let abscissa of any point P of the curve OQP represent V, ordinate $f(V)$. If $K_p > K$ area ONPQO > area of triangle ONP, *i.e.*, unless the curve $y = (fx)$ has a point of inflexion between O and P, the fact that $K_p > K$ implies that K increases with V,—a conclusion again at variance with experimental results.

We are thus unable to account for the observation on the hypothesis that the capacity varies inversely as x. Let us now suppose that $f(V) = V\phi(x)$, that is to say, that however the capacity may depend on the distance, it is independent of the charge, or is constant for any

given condenser. It at once follows that $K_s = K$, which is discordant with observation. Consider, however, the ratio—

$$K_p/K = \int_0^V \frac{dV}{dx} f(V) dV \Big/ \frac{1}{2}\frac{V}{x} f(V) = -\frac{\phi'(x)}{\frac{\phi(x)}{x}},$$

since $0 = \frac{dV}{dx}\phi(x) + V\phi'(x)$ when e is constant. Suppose $\frac{K_p}{K} = m$, a positive constant quantity greater than unity—

$$m\phi(x) + x\phi'(x) = 0,$$

$$x^m\phi(x) = C, \text{ a constant as regards } x,$$

or

$$\phi(x) \propto x^{-m}.$$

We could, therefore, account for K_p being greater than K by supposing that the potential difference with given charge per unit of area does not vary as x but as x^m. Such a supposition would be subversive of all accepted ideas of electrostatics.

There remains one other consideration to be named. We have assumed throughout that the charge of the condenser depends only on the distance of the plates and their difference of potential, and is independent of previous charges or of the time the difference of potential has existed. We have ignored residual charge. It is easy to see what its effect will be on determinations of K made by measuring the potential and charge of the condenser. It is not so obvious what its effect will be in all cases on the force between the plates. Consider a complete cycle of operations: the condenser is charged with quantity e, the distance between the plates is increased from x to $x+dx$, the condenser is discharged and the plates return to their initial position. The work done respectively in charging the condenser, separating the plates, and recovered in discharging the condenser, will depend on the rate at which these operations are performed. There are ideally two ways of performing them, so that no energy is dissipated by residual charge, first, under certain reservations, so rapidly that no residual charge is developed; second, so slowly that at each potential the residual charge is fully developed; in either case the potential is a function of the then charge, and not of the antecedent charges. The attraction between the plates will differ according as the charge is an instantaneous one or has been long applied. If a liquid were found exhibiting a considerable slowly developed residual charge, the capacity determined by attraction with continuous charge would be greater than the capacity determined by an instantaneous discharge of the condenser through a galvanometer or into another condenser.

I am not aware that residual charge has been observed in any liquid dielectric.

The results obtained by Professor Quincke are not easy to reconcile. For that reason it is the more desirable that their full significance should be ascertained. Full information is given of all the details of his experiments except on one point. It is not stated whether, in the experiments for determining K by direct discharge of the condenser, the capacity of the connexion and key was ascertained. It would in most ordinary arrangements of key be very appreciable in comparison with the capacity of the condenser itself. If neglected the effect would be to a certain extent to give too low a value of K, the effect being most marked when K is large.

I have made a few preliminary experiments to determine K for colza oil with several different samples, and both with continuous charges and intermittent charges from an induction coil. The values of K range from 2·95 to 3·11. Professor Quincke's results in his first paper are $K=2·443$, $K_p=2·385$, $K_s=3·296$.

The property of double refraction in liquids caused by electrification is sometimes cited as showing that electrification is not proportional to electromotive force. The fact that the double refraction in a liquid under powerful electromotive forces is very small would further show that there is a close approximation to proportionality, and that the deviation from proportionality would be insensible to any electrostatic test. Such conclusions, however, cannot be safely drawn in the case of bodies such as castor-oil, in which $K \pm \mu^2$. In such bodies, assuming the electromagnetic theory of light, the yielding to electromotive force is much greater if the force be applied for such time as 10^{-4} second than when applied for 10^{-14} second, and it is quite possible that the law of proportionality might be untrue in the former case, but very nearly or quite true in the latter.

"Addendum to Dr. Hopkinson's 'Note on Specific Inductive Capacity.'" By Professor QUINCKE, For. Mem. R.S. Received December 5, 1886.

Notiz über die Dielectricitätsconstante von Flüssigkeiten, von G. Quincke.

Bei Gelegenheit einer Untersuchung der Eigenschaften dielectrischer Flüssigkeiten ('Wiedemann, Annalen,' vol. 19, 1883, p. 707; vol. 28 1886, p. 529) hatte ich die Dielectricitätsconstante mit der electrischen Wage oder dem hydrostatisch gemessenen Druck einer Luftblase grösser gefunden, als mit der Capacität eines Condensators, der von Luft oder isolirender Flüssigkeit umgeben ist, und beim Umlegen eines Schlüssels durch einen Multiplicator entladen wird.

Die Capacität des Schlüssels und des kurzen dünnen Verbindungsdrahtes, welcher den Schlüssel mit dem Condensator verband, wurde aber dabei als verschwindend klein vernachlässigt.

In Folge einer brieflichen Mittheilung von Herrn Dr. John Hopkinson habe ich in neuster Zeit die Capacität des Schlüssels und des Zuleitungsdrahtes mit der Capacität C des Condensators durch Multiplicator-Ausschläge bei derselben Potentialdifferenz der Belegungen verglichen und dabei das Verhältniss—

$$\frac{x}{C} = 0{\cdot}1762$$

gefunden, also viel grösser als ich vermuthet hatte.

Zieht man von den beobachteten Multiplicator-Ausschlägen s_1 und s_{11} für den Condensator in Luft und in der dielectrischen Flüssigkeit den Ausschlag ab, der von der Electricität auf dem Schlüssel und Verbindungsdraht herrührt, so erhält man in der That durch das Verhältniss der so corrigirten Ausschläge (s_1) und (s_{11}) Werthe der Dielectricitätsconstante (K) der Flüssigkeit, die fast genau mit den Messungen der electrischen Wage übereinstimmen. Die Uebereinstimmung ist so gross, wie bei der Verschiedenheit der benutzten Beobachtungsmethoden nur erwartet werden kann.

So ergab sich z. B.

	Dielectricitätsconstante mit	
	Multipl. (K)	Wägung. K_p
Aether	4·211	4·394
Schwefelkohlenstoff	2·508	2·623
„	2·640	2·541
Benzol	2·359	2·360
Steinöl	2·025	2·073

Heidelberg, December 1, 1886.

[Note added Dec. 4th.—Professor Quincke's explanation sets the questions I have raised at rest. There can be little doubt that K, K_s and K_p are sensibly equal and sensibly constant. The question what will happen to K_p and K_s if K is not constant has for the present a purely hypothetical interest.—J. H.]

II. "On a Varying Cylindrical Lens." By TEMPEST ANDERSON, M.D., B.Sc. Communicated by Professor A. W. WILLIAMSON, For. Sec. R.S. Received November 18, 1886.

A cylindrical lens of continuously varying power has long been a desideratum, and one was constructed and described by Professor Stokes, at page 10 of the Report of the British Association for 1849 (Transactions of the Sections). He points out that—

"If two plano-cylindrical lenses of equal radius, one concave and the other convex, be fixed, one in the lid and the other in the body of a small round wooden box, with a hole in the top and bottom, so as to be as nearly as possible in contact, the lenses will neutralise each other when the axes of the surfaces are parallel; and by merely turning the lid round an astigmatic lens may be formed, of a power varying continuously from zero to twice the astigmatic power of either lens."

This very beautiful optical contrivance has the disadvantage that the refraction varies from zero in both directions at once, the refraction at any given position of the lenses being positive in one meridian, and negative or concave to an equal degree in a meridian at right angles to the first; moreover, there is no fixed axis in which the refraction is either zero or any other constant amount. It has in consequence never come into extensive use in the determination of the degree of astigmatism. The author has planned a cylindrical lens in which the axis remains constant in direction and amount of refraction, while the refraction in the meridian at right angles to this varies continuously.

A cone may be regarded as a succession of cylinders of different diameters graduating into one another by exceedingly small steps, so so that if a short enough portion be considered, its curvature at any point may be regarded as cylindrical. A lens with one side plane and the other ground on a conical tool is therefore a concave cylindrical lens varying in concavity at different parts according to the diameter of the cone at the corresponding part. Two such lenses mounted with axes parallel and with curvatures varying in opposite directions produce a compound cylindrical lens, whose refraction in the direction of the axes is zero, and whose refraction in the meridian at right angles to this is at any point the sum of the refractions of the two lenses. This sum is nearly constant for a considerable distance along the axis so long as the same position of the lenses is maintained. If the lenses be slid one over the other in the direction of their axes, this sum changes, and we have a varying cylindrical lens. The lens is graduated by marking on the frame the relative position of the lenses when cylindrical lenses of known power are neutralised.

It was found by a practical optician to be impossible to work glasses on a cone of large diameter, consequently a conical tool was constructed with an angle of 45° at the apex, and 8 inches diameter at the base.

A glass about 4 inches long was ground on the sides of this near the base, and as the resulting lens if ground on plane glass would have been too concave for most purposes, the outer side of the glass was previously ground to a convex cylindrical curve, and its axis applied parallel to the generating line of the cone in the plane of the axis of the cone.

The result was concavo-convex cylinders of varying power suitable for the practical measurement of astigmatism.

Lenses were exhibited varying from 0 to − 6DCy, and from 0 to + 6DCy.

III. "On the Action of the Excised Mammalian Heart." By AUGUSTUS WALLER, M.D., and E. WAYMOUTH REID, M.B. Communicated by Prof. BURDON SANDERSON, F.R.S. Received November 18, 1886.

(Abstract.)

The graphic method, the galvanometer, and the capillary electrometer were made use of in this research. The animals used were the dog, rabbit, cat, rat, guinea-pig, and sheep. The chief results were as follows:—

1. Spontaneous ventricular contractions, complete and capable of being recorded, continue after excision of the heart for periods which are variable, but which as a rule are longer than has generally been received to be the case (Czermak and Piotrowsky).

2. Spontaneous ventricular contractions frequently outlast auricular contractions, both spontaneous and excited.

3. After spontaneous ventricular contractions have ceased to occur, electrical and mechanical excitations can still provoke contraction.

4. The length of contraction of both auricle and ventricle of the excised heart is very great (15 to 20 times the normal duration), whether the contraction be spontaneous or excited.

5. The length of the latent period increases with the length of contraction; it may be as long as 0·75 sec.

6. These phenomena (4 and 5) depend principally upon the surrounding temperature.

7. The heart (of a rabbit) can regain its excitability and its power of spontaneous contraction after it has been frozen hard.

8. In an excited beat, in ventricle or auricle, of an excised heart, all parts are not in action simultaneously.

9. An excited contraction starts from the point excited—from the base if the base is stimulated—from the apex if the apex is stimulated.

10. The excited contraction travels from its point of origin, indifferently in any direction in the substance of the ventricle.

11. The rate of the wave of contraction measured by the graphic method varies, according to the temperature and state of the heart, from 3 to 85 cm. per second.

12. The velocity is (*cæteris paribus*) greater in the hearts of large than in the hearts of small animals.

13. In spontaneous contractions of the ventricle the movement of the apex appears to precede that of the base.

Note.—We have followed the wave of both spontaneous and excited contraction of the frog's ventricle by the graphic method, and measured its rate. The rate is from 40 to 80 mm. per second. In spontaneous contraction, the movement of the base precedes that of the apex.

14. All parts of the uninjured heart are iso-electric; the apex, however, is often slightly negative to the base.

15. The electrical variation of spontaneous contraction is sometimes diphasic (corresponding to the double variation of the beat of the frog's heart), sometimes monophasic.

16. Electrical variations can be detected after visible contractions have ceased.

17. The direction of the electrical current follows no definite rule in our observations, negativity of apex preceding sometimes negativity of base, at other times the reverse taking place.

18. The diphasic variation of excited contractions indicates that the part stimulated is first negative then positive to other parts.

19. Under conditions of lowered excitability a weak excitation will give a monophasic, a strong excitation a diphasic variation.

20. Towards the close of the period of excitability, the variation of the excited beat is single. It is less frequently single at the outset of experiment.

21. When excitatory variations can no longer be obtained, injury will produce a change of electrical state indicating negativity of the part injured.

IV. "On the Effect of Polish on the Reflexion of Light from the Surface of Iceland Spar." By C. SPURGE, B.A., St. Catherine's College, Cambridge. Communicated by R. T. GLAZEBROOK, M.A., F.R.S. Received November 18, 1886.

(Abstract.)

The most complete experiments which have been hitherto instituted to determine the optical effect of polishing the surface of a transparent body are those of Seebeck, described in 'Poggendorff, Annalen,' vol. 20, 1830, p. 27; vol. 21, 1831, p. 290. Seebeck's method was to quench, as far as possible, the reflected light with a Nicol, and to measure the angle of incidence. It was from a change in the angle of incidence, *i.e.*, the angle of polarisation, that an alteration of the state of the surface was inferred.

This method is open to the objection that the light is not completely quenched, and, therefore, since the effect of polishing as observed by Seebeck was not very large, our conclusions as regards the surface state may be modified. Besides, the investigation is incomplete, for the precise change produced in the reflected light by polishing is not determined.

The present paper, of which this is an abstract, is an account of experiments made to determine with greater accuracy the effect of polishing the surface of a crystal of Iceland spar, and also the exact alteration produced in the reflected light, *i.e.*, the change in the ratio of the axes of the ellipse, and in the azimuth of the major axis of the elliptically polarised light.

To effect this, an elliptic analyser, consisting of a Nicol and a quarter undulation plate, was employed. If r, r' be the mean readings of moveable verniers attached to the Nicol in the two distinct positions in which the light is extinguished, and R, R′ similar readings of fixed verniers which determine the azimuth of the quarter plate, then $\tan\varpi$, the ratio of the axes, and I, the azimuth of the major axis of the elliptically polarised light, were calculated from the formulæ—

$$\cos 2\varpi = \sin(r'-r)/\sin(R'-R) \text{ and } I = (R+R')/2.$$

Especial care was taken to secure fixity of position in all permanent parts of the instrument, and in the setting of the face of the crystal, since a small change was to be detected.

A first series of experiments was made with light reflected from a natural face, and with light reflected from the same face when polished. The polishing was performed by *myself*, and precautions

were taken that the polishing was effected under exactly the same conditions. To ensure that the face was polished parallel to itself, the inclinations of the face to fixed faces were measured before and after polishing. The mean of about 400 readings was taken with each state of the face of the crystal, to obtain as accurate a result as possible.

A second series of experiments was instituted with a view to determine the accuracy with which the means of each of the previous sets of observations could be determined. At the same time, a simple analyser, consisting of a Nicol and a graduated circle, was set up in addition to the elliptic analyser, with the object of testing the conclusions of Sir John Conroy as regards polished surfaces, which are given in the 'Proceedings of the Royal Society' for February, 1886. The effect of rotation of the face of the crystal through a small angle in its own plane is also discussed. The observations are divided into sets, the means of which are compared with each other.

Lastly, a set of experiments was made with a cleavage face split off near the former, and the results of these experiments were compared with those of the first series made some fifteen months previously.

The results of the experiments are embodied in tables, from which the following numerical results are extracted. I′ in the case of the simple analyser corresponds to I for the elliptic analyser.

The general conclusions of the paper are as follows:—

The process of polishing the surface of a crystal of Iceland spar with emery and rouge does most certainly alter the state of the surface. This alteration is evinced by a change both in the ratio of the axes and in the azimuth of the major axis of the elliptically polarised light. Such an alteration was observed in the case of two different crystals which were made the subject of experiment.

The light reflected is shown to be exceedingly nearly plane polarised, so that the absolute amount of change in the ratio of the axes is small; but the relative change is considerable, for $\tan \varpi$ is changed from 0·0334 to 0·0252. The change in the azimuth of the major axis is not very large. As regards disturbing causes, it is proved that moderate changes of temperature do not cause any very perceptible alteration in the surface state. The experiments prove a result unnoticed by Seebeck, that an emery-rouge polished surface gives perfectly concordant results on repolishing, and in this respect is quite as satisfactory as the chalk-polished surface that Seebeck recommends. And in general the results of the paper tend to confirm the views of Seebeck rather than those of Sir J. Conroy, for Seebeck in his paper prefers polished surfaces because of the liability of the natural surface to tarnish.

		Elliptic analyser.			Simple analyser.	
		tan ϖ.	I.	Readings.	I′.	Readings.
Ist Series.....	Natural face.	0·03345	108° 5·3′	416	—	—
	Same face polished ..	0·02517	107 49·1	384	—	—
IInd Series. (Elliptic analyser reset).....	Same face polished ..	0·02655	107 49·1	448	266° 5·3′	100
	Same face *re*-polished ..	0·02723	107 52·6	1280	265 57·3	500
	The first polished face rotated thro' 4° 27′	0·03305	107 39·2	960	—	—
IIIrd Series. (Base of crystal broken up by cleavage).....	Natural face.	0·03368	108 31·4	384	—	—
Effect of time ..	Polished face Dec. 8	—	—	—	111 15·7	40
	Polished face Jan. 20 ...	—	—	—	111 16·7	60
	.....	—	—	—	111 16·4	60

V. "Contributions to the Chemistry of Chlorophyll. No. II." By EDWARD SCHUNCK, F.R.S. Received November 25, 1886.

(Abstract.)

In this paper the author continues his account of the properties of phyllocyanin, one of the products of the action of acids on chlorophyll. He shows that by passing a current of CO_2 through an alcoholic solution of phyllocyanin holding oxide of zinc in suspension, a compound is obtained containing zinc and carbonic acid, a phyllocyanin zinc carbonate resembling phyllocyanin zinc acetate, but that no analogous compounds containing iron or copper are formed in this way. Attention is directed to the points of resemblance between the double compounds of phyllocyanin containing zinc and chlorophyll itself, particularly as regards their susceptibility to change when exposed to the action of air and light, and it is shown that while

these compounds when in solution lose their colour almost as rapidly as chlorophyll itself, those containing copper are remarkably stable, since their solutions may be exposed for many weeks to light and air without undergoing any apparent change.

The products derived from phyllocyanin by reduction are next described. The action of tin and hydrochloric acid on phyllocyanin passes through two distinct stages. During the first stage a colouring matter is formed which is remarkable from its solutions showing no less than eight absorption bands. The product formed during the next stage of the process is interesting from its yielding solutions of a bright red colour without any tinge of green, and from its resembling in some respects the colouring matters of red flowers.

VI. "On the Changes in the Proteids in the Seed which accompany Germination." By J. R. GREEN, B.Sc., B.A., Demonstrator of Physiology in the University of Cambridge. Communicated by Professor M. FOSTER, Sec. R.S. Received November 25.

(Abstract.)

The processes of the germination of the seed have been in recent years investigated by v. Gorup-Besanez, who in a series of papers written in 1874 and 1875,* has stated that the changes in the reserve proteid materials are probably due to the action of a proteolytic ferment, as from the seeds of the vetch, hemp, flax, and barley plants he was able to extract a body which converted fibrin into peptone. Later, in 1878, Krauch† disputed v. Gorup-Besanez's conclusions, and claimed that his results were erroneous on account of imperfect methods of working. As v. Gorup-Besanez based his statement partly on the detection of peptone by the biuret test after the digestion had gone on for some time, and partly on a diminution of the fibrin, Krauch explained his results by saying that the digestive extract itself gave a biuret reaction, and that the diminution of the fibrin was only due to a shrinkage of its flocks.

During the past year I have been carrying out a series of experiments bearing upon this disputed point, and have succeeded in demonstrating in the seeds of the lupin (*Lupinus hirsutus*) the existence of such a ferment as v. Gorup-Besanez stated to be present, and in ascertaining some particulars as to its condition in the resting seed, the nature and conditions of its action, and the changes which

* 'Deutsch. Chem. Gesell. Ber.,' 1874, p. 1478. *Ibid.*, 1875.

† "Beiträge zur Kenntniss der ungeformten Fermente in den Pflanzen," 'Landwirthsch. Versuchs-Stat.,' vol. 27, 1878, p. 383.

it brings about not only in the course of its action on fibrin, but also on the aleurone or proteid reserve material in the seed itself.

The method which I used in the investigation was somewhat different from that of v. Gorup-Besanez. As Krauch claimed that the biuret reaction obtained was due to some proteid in the digestive extract used, and as the vegetable peptones found in the seed of the lupin do not dialyse, while true peptone does so readily, I carried on my digestions always in carefully tested tubes of dialysing paper, so that the fluid outside the latter might enable me to see if peptone were really formed or no.

Seeds of *Lupinus* were germinated for about a week, till they had protruded a radicle of about $1\frac{1}{2}$ inch in length; they then gave an acid reaction to litmus-paper. They were divested of their coats, the radicles removed, and the cotyledons ground. The resulting powder was extracted with glycerine, and the extract dialysed till no trace of any crystalline bodies that had been formed during the germination could be detected in the dialysate. No trace of peptone or other body giving a biuret reaction passed the dialyser, even after a week's exposure. The extract was then acidified with HCl to the extent of 0·2 per cent., put into a fresh dialyser, some swollen-up boiled fibrin added, and the dialyser put into a beaker and surrounded with 0·2 per cent. HCl. It was then left at a temperature of 40° C. Control experiments, some with boiled digestive extract, some with 0·2 per cent. HCl only, were carried out side by side with the others.

The process of digestion was very slow, the time taken up being very much more prolonged than is the case with the gastric or pancreatic ferments. After some time, however, the dialysate in the beaker containing the tube in which the unboiled extract of the cotyledons had been placed gave a very marked biuret reaction, and after concentration it deposited crystals of leucin. The other dialysates contained no peptone or crystalline body.

I repeated the experiments many times with varying quantities of the extract of the cotyledons and with varying amounts of fibrin, and in all cases I was able to see that a proteolytic ferment was present in the germinating seed, and that it formed not only peptone but leucin, behaving like pancreatic rather than gastric juice. In this latter particular I am somewhat at variance with v. Gorup-Besanez, who says he was not able to see that the decomposition of the fibrin proceeded beyond the stage of peptone.

Further investigations into the condition of the action of the ferment showed that it worked best in a medium acidified to the extent of 0·2 per cent. HCl: that the temperature most favourable for its working was 37—40° C., that its activity was somewhat impeded by the presence of excess of neutral salts, and that it was speedily destroyed by contact with alkalis, even to the extent of 1 per cent.

In the resting seed the ferment exists in the form of a zymogen, as is the case with those of the stomach and other digestive organs. This is, however, very readily converted into the active ferment by contact with dilute acids. The point was somewhat difficult of proof, but was ascertained by a modification of the method adopted by Langley and Edkins* in their work on the condition of the ferment in the gastric glands. From this paper it appears that while both alkalis and CO_2 destroy both the ferment and the zymogen, the latter is much more easily affected by CO_2 than the former. I found this to be the case with the extracts of the seeds. After a stream of CO_2 had been passed through them, treatment with acid failed to make them active, though the acid soon developed ferment-power in extracts not treated with the gas. The reaction of the resting seeds was neutral.

The proteids existing in the seeds of *Lupinus* have been ascertained by Vines† to consist of hemialbumose and globulin. I prepared from a quantity of the resting seeds a considerable bulk of these by the methods Vines describes, and submitted them in a state of fair purity to the action of the ferment. The outcome of a long series of experiments so carried out was that the proteids of the seeds were changed by the ferment in much the same way as fibrin. There was soon a quantity of parapeptone formed, which was soluble readily in weak acids or alkalis. This was followed or accompanied by the appearance of peptone, and later, leucin and asparagin were formed. The latter bodies were obtained in some quantity by the method described by v. Gorup-Besanez‡ for the separation of leucin from other bodies in the fluids in which it is found.

This course of digestion of the seed proteids was confirmed by examination of the seeds at different stages in their natural germination. In those which were just starting, parapeptone in quantity was present in the germinating cotyledons; a little later abundance of peptone was found. In no case was peptone found in the radicles, but from these plenty of asparagin was easily obtainable.

Besides the biuret test for the peptone a more delicate one was generally used, which has been described by many writers. It consists in freeing the solution from all other proteids by boiling with freshly prepared ferric acetate and then adding to it acetic acid and phosphotungstate of soda. Peptone is then precipitated.

The conclusions which seem to follow from the whole course of the experiments are:—

1. There exists in the seed of the lupin when germinating a proteo-

* 'Journal of Physiology,' vol. 7, p. 371 (1886).

† 'Journal of Physiology,' vol. 3, p. 93 (1881).

‡ 'Anleitung zur qualitativen und quantitativen Zoochemischen Analyse.'

lytic ferment which will convert fibrin into peptone and then into leucin and tyrosin.

2. This exists in the resting seed in the form a zymogen, which is easily convertible into the ferment.

3. The ferment acts best in a slightly acid medium; its activity is hindered by neutral salts and destroyed by alkalis, and it is most active at a temperature of 40° C.

4. The process of germination is started or accompanied by a transformation of the zymogen into ferment on the absorption of water and the development of vegetable acids in the cells of the seed.

5. The ferment so developed converts the proteids of the resting seed into acid albumin or parapeptone, peptone, and crystalline amides.

6. The nitrogen travels from the cells of the seed to the growing points in the form of the latter bodies and not in that of peptone or other proteid.

VII. "Preliminary Account of the Observations of the Eclipse of the Sun at Grenada in August, 1886." By Captain Darwin, R.E. Communicated by Lord Rayleigh, Sec. R.S. Received November 25, 1886.

The instruments allotted to me consisted of the coronagraph and the prismatic camera; the two instruments being mounted on the same equatorial stand.

The prismatic camera is the same instrument which was used at the eclipses of 1882 and 1884. It consists of an ordinary photographic camera with a 60° prism placed in front of the lens.

The coronagraph consists of a reflecting telescope arranged for obtaining photographic records, and in which special precautions are taken to avoid internally reflected light.

This instrument was designed by Dr. Huggins, with the idea that it might be possible to obtain photographs of the corona in sunlight, that is at other times than at eclipses, and I was especially directed to test the practicability of this method. The test could be applied in two ways:—

1st. By obtaining photographs shortly before or after the eclipse, and comparing any irregularity that might appear in the halo round the sun with any photographs of the corona taken during totality; a similarity of form indicating that the corona had been photographed.

2nd. To take photographs during partial eclipse. Then if the light of the corona produces any effect on the plate, the limb of the moon should be visible against it.

On the day before the eclipse I took a considerable number of photographs for the first test. No similarity has yet been traced between the form of the corona as obtained on these plates, and the form of the true corona as obtained during the total eclipse.

During totality I had intended to carry out the following programme :—During the first minute a photograph was to be taken with the prismatic camera. After that four plates were to be exposed with the coronagraph with the same length of exposure as that given during sunlight. The exposure was given automatically by means of a shutter, with an estimated length of between one-tenth and one-fifth of a second. Besides these, two photographs were to be taken with exposures of five and ten seconds respectively.

The programme could not be carried out exactly. Immediately after I had commenced exposing the prismatic camera, I looked up, and found that the corona was covered by a light cloud. The sky became clear again in about fifty seconds. I was anxious not to take any other photographs at the same time for fear of vibration ; but as nearly a minute had been lost something had to be sacrificed, and I decided to take some of the photographs with the coronagraph before putting the cap on the prismatic camera. I do not think that the work has suffered in consequence, and at all events I obtained all the plates I had intended to. As to the results, I am not yet in a position to fully report on them.

The photograph obtained with the prismatic camera shows several images of the prominences, and it therefore gives every promise of yielding good results when measured and examined.

The five and ten second photographs of the corona show signs of a slight vibration, but they will be useful for the inner part of the corona. As my main object was to obtain instantaneous photographs, these long exposure plates had to be obtained by working the automatic shutter by hand ; it was this probably that caused the vibration.

The instantaneous photographs of the corona when developed were complete blanks, proving that the exposure was too short. It should, however, be observed that this does not prove that the light of the corona was insufficient to cause an appreciable effect on the plate if combined with other light. More light energy is necessary to start photographic action than is required to produce a visible difference of shade when once the action is started.

Many of the photographs taken during partial eclipse show what may be described as a false corona, that is, an increase of density near the sun and between the cusps, or *in front of* the moon. In none of them can the moon be seen eclipsing the corona.

The results, therefore, are adverse to the possibility of obtaining photographs of the corona in sunlight ; it is, however, I consider by no means proved that the method is impossible. But at present I am

inclined to consider that the result tends to show that a *practical* method of obtaining photographic records of the corona during sunlight is not likely to be obtained. The trial was not conclusive because the conditions were very unfavourable. In order to reduce the air-glare to a minimum, so that the light of the corona shall not be overpowered, the following points must be observed :—

1st. The air should be clear and dry.

2nd. The sun should be near the zenith.

3rd. The station should be at a considerable elevation above the sea.

4th. The corona, if it does vary in intensity, should be at its maximum brightness.

Now every one of these conditions was unfavourable. The air was saturated with moisture, the sky was of a hazy blue, the sun was low, the station was near the sea-level, and the corona according to the general impression was not so bright as on other occasions.

I hope, however, to deal more fully with the considerations on another occasion.

The Society adjourned over the Christmas Recess to Thursday, January 6th, 1887.

Presents, December 9, 1886.

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OBITUARY NOTICES OF FELLOWS DECEASED.

JAMES APJOHN, M.D., was Professor of Chemistry in the University of Dublin from 1850 to 1875. He died on the 2nd of June, 1886, at Southill, Blackrock, co. Dublin, in his 91st year. Dr. Apjohn was born at Sunville, co. Limerick, on the 1st of September, 1796, and, having received his elementary education at the Tipperary Grammar School, he entered Dublin University in 1814. After a distinguished undergraduate course, he took his Arts degree in 1817, and in 1821 obtained the M.B. Sixteen years later he proceeded to the M.D. degree. Apjohn's love for experimental science seems to have been kindled during his medical studies, and after graduating he devoted himself almost exclusively to the pursuit of chemistry and physics. About 1824 several eminent physicians and surgeons—including Groves, Marsh, Jacob, and Cusack—decided to establish a new medical school at Parke Street, in Dublin, and in the following year Apjohn joined the staff of the new school as lecturer in chemistry. Here he acquired considerable reputation as a lecturer, and three years later he was transferred to the newly-established Chair of Chemistry in the College of Surgeons' School, which he held until 1850, when he was appointed to the University Chair of Chemistry on the death of Dr. Francis Barker. Apjohn's official connexion with the University began, however, in 1841. About that time the Board of Trinity College founded an Engineering School, and appointed Apjohn to the Chair of Applied Chemistry and Mineralogy—an office which he continued to hold when he succeeded Barker in 1850. On Dr. Apjohn's retirement from professorial work, Mineralogy was transferred to the Chair of Geology in the University, and Applied Chemistry was permanently attached to the Chair of Chemistry.

The principal scientific work of Dr. Apjohn was rather physical than chemical. The general study of hygrometry had a special attraction for him, and of the forty-nine scientific papers named in the Royal Society's Catalogue, a large proportion relates to that subject. In the course of his work on the theory of the wet bulb hygrometer, he arrived at the expression well known as "Apjohn's Formula" for ascertaining the dew point.

The study of hygrometry led to much interesting work on the specific heat of gases, and in 1837 Apjohn received the Cunningham

Medal from the Irish Academy for his paper "Upon a New Method of Investigating the Specific Heats of the Gases." The method consisted in noting the fall in temperature suffered by the wet bulb thermometer when immersed in the perfectly dry gas, whose specific heat was required. From 1838 onwards Dr. Apjohn devoted less attention to physical work than to mineralogical chemistry. He was a frequent contributor to the literature of the latter subject. In 1838 he analysed and described a mineral from Algoa Bay, South Africa, which proved to be a somewhat efflorescent manganese alum, which has since been named "Apjohnite." In 1840 he described a mineral found at Kilbricken, co. Clare, which is closely related to geocronite, but is non-arsenical. Again, in 1852 he was concerned in the description, under the name of "Jellettite," of a yellowish-green garnet found near Zermatt, Monte Rosa, by Dr. Jellett, the present Provost of Trinity College. Papers on the relations of pyrope, of pennine, and on Mexican hyalite are also to be found amongst his contributions to mineralogy.

Dr. Apjohn contributed to the "Cyclopædia of Practical Medicine" the articles on electricity, galvanism, toxicology, and spontaneous combustion, and it is stated that the latter article supplied Dickens with the facts on which he founded his account of Krook's death in "Bleak House."

As the representative of the University of Dublin in the General Medical Council, Dr. Apjohn took a prominent part in the production of the "British Pharmacopœia." Almost every chemical process and test described in the first edition of the "Pharmacopœia" was carefully examined in the Trinity College Laboratory, and much of the success of the work was due to Apjohn's laborious revision in detail.

He was elected to the Fellowship of the Royal Society in 1853; he was a Vice-President of the Royal Irish Academy; and at the time of his death was second on the roll of Fellows of the King and Queen's College of Physicians.

Dr. Apjohn was widely esteemed throughout his long life as a thorough and earnest worker, a singularly lucid and able lecturer, and an upright and honourable man.

J. E. R.

William Benjamin Carpenter was born at Exeter in 1813, and was the fourth child and eldest son of Dr. Lant Carpenter, a Unitarian minister. His sister, Mary Carpenter, who died a few years since, achieved the most important work as a philanthropist, in relation to the treatment of prisoners and to questions affecting our Indian fellow-subjects, and will be remembered by future generations with no less gratitude than her brother.

In his childhood Dr. Carpenter received an excellent education,

comprising both classics and the principles of physical science, at the school established by his father at Bristol, and it was his intention to adopt the profession of a civil engineer. He was, however, persuaded to accept the opportunity offered by a medical practitioner, Mr. Estlin, of Bristol, and to enter on the study of medicine as apprentice to that gentleman. Shortly after this he was sent, as companion to one of Mr. Estlin's patients, to the West Indies, and on his return from this visit he entered, at the age of twenty, the medical classes of University College, London. After passing the examinations of the College of Surgeons and the Apothecaries' Society he proceeded to Edinburgh, where he graduated as M.D. in 1839.

His graduation thesis on "The Physiological Inferences to be deduced from the Structure of the Nervous System of Invertebrated Animals" excited considerable attention, especially on account of the views which he advanced as to the reflex function of the ganglia of the ventral cord of Arthropoda.

From the first Dr. Carpenter's work showed the tendency of his mind to seek for large generalisations and the development of philosophical principles. He was a natural philosopher in the widest sense of the term—one who was equally familiar with the fundamental doctrines of physics and with the phenomena of the concrete sciences of astronomy, geology, and biology. It was his aim, by the use of the widest range of knowledge of the facts of Nature, to arrive at a general conception of these phenomena as the outcome of uniform and all-pervading laws. His interest in the study of living things was not therefore primarily that of the artist and poet so much as that of the philosopher, and it is remarkable that this interest should have carried him, as it did, into minute and elaborate investigations of form and structure. Although some of his scientific memoirs are among the most beautifully illustrated works which have been published by any naturalist, yet it is noteworthy that he himself was not a draughtsman, but invariably employed highly skilled artists to prepare his illustrations for him. Yet we cannot doubt that the man who, with his dominant mental tendency to far-reaching speculations, yet gave to the world the minute and ingenious analysis of the beautiful structure of the shells of Foraminifera, had an artist's love of form, and that the part of his life's work (for it was only a part among the abundant results of his extraordinary energy) which was devoted to the sea and the investigation of some of its fascinating living contents, was thus directed by a true love of Nature in which ulterior philosophy had no share.

Two books, Dr. Carpenter has told us, exerted great influence over his mind in his student days: they were Sir John Herschel's "Discourse on the Study of Natural Philosophy" and Lyell's "Principles of Geology"—that great book to which we owe the even greater books

of Charles Darwin. Taking the "Principles" in some way as his model, Dr. Carpenter produced in 1839 his first systematic work, under the title "Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology and as a guide to the Philosophical Pursuit of Natural History." Admirable as was the execution of this work in many ways, its great merit lay in the conception of its scope. It was in fact the first attempt to recognise and lay down the lines of a science of "Biology" in an educational form. Carpenter's "Comparative Physiology" is the general or elementary "Biology" of the present day—traced necessarily upon the less secure foundations which the era of its production permitted, viz., one year only subsequent to the date of Schwann's immortal "Microscopical Researches."

For five years Dr. Carpenter remained in Bristol, commencing medical practice and marrying in 1840; but in 1844, feeling a distaste for the profession of medicine, he moved to London in order to devote himself entirely to a literary and scientific career. He was encouraged to take this step by the success which his "Comparative Physiology" obtained, a second edition having been called for within two years of the publication of the first. He was appointed Fullerian Professor of Physiology in the Royal Institution during his first year in London, and Professor and Lecturer at University College and at the London Hospital, whilst he was also elected a Fellow of the Royal Society.

In 1851 Dr. Carpenter became Principal of University Hall, the residential institution attached to University College, where he remained until 1859. During this period he remodelled his treatise on Physiology, issuing the more general biological portion as "Comparative Physiology," whilst that portion dealing with the special physiology of man and the higher animals appeared as his well-known "Human Physiology," which subsequently ran through many editions. The "Human Physiology" is remarkable in the first place for the chapters on the physiology of the nervous system, and especially for the theories enunciated with regard to the relations of mind and brain, and the attempt to assign particular activities to particular portions of the cerebral structure. In arriving at his conclusions Dr. Carpenter had to depend on arguments drawn from the facts of comparative anatomy and of diseased or abnormal conditions in man. There is no doubt at the present day of the acuteness which he displayed in his treatment of the subject, and of the truth in a general way of the results which he formulated. Experiment and a wider range of observation have to some extent corrected—but on the whole rather extended and confirmed—the doctrines of the early editions of the "Human Physiology" in regard to this subject, so that he was able only a few years since to separate this portion of the work and issue it as a separate book, the "Mental Physiology," in which is contained by far

the most complete, consistent, and readable account of the phenomena of mind, and their relation to the actual structure of the brain, which exists. Such topics as Instinct, Mesmerism, Somnambulism, Unconscious Cerebration (his own phrase), &c., are discussed in a masterly way, and with an abundance of illustration and knowledge which renders the work one of the greatest value even to those who may differ here and there from its theoretical conclusions.

About the period of his removal to London Dr. Carpenter began to occupy himself with the minute study of the structure of the calcareous shells of the Mollusca—being led thereto by a desire to compare the results of the operation of living matter upon distinctly mineral compounds (such as carbonate of lime), by way of comparison and in illustration of the rapidly accumulating knowledge of cell-structure in the softer parts of living things. This study, which resulted directly in some valuable contributions to our knowledge of the structure of shells, shown by these researches to be far more complex than had hitherto been supposed, led on the one hand to Dr. Carpenter's permanent identification with the pursuit of research with the microscope, and on the other hand to those admirable investigations of the structure and law of growth of the shells of the minute Protozoic Foraminifera which constitute his most weighty contribution to the special literature of science. His microscopic studies bore fruit in the publication of "The Microscope and its Revelations," the sixth edition of which was issued in 1881. The studies on the shells of Foraminifera were continued throughout his life, being published in four memoirs in the "Philosophical Transactions," and in a richly illustrated monograph produced by the Ray Society in 1862, whilst the last of his memoirs in the "Philosophical Transactions" was that on Orbitolites bearing date so late as 1882. It was on this subject that Dr. Carpenter was busy at the time of his death, having during the past few years accumulated a wealth of material and drawings in support of his contention that the *Eozoon canadense* discovered by Logan in the Laurentian rocks of Canada exhibits the distinctive structure of the shell-substance of the higher Foraminifera. The material relating to Eozoon has been placed by Dr. Carpenter's executors in the hands of Mr. Rupert Jones, who has undertaken to prepare it for publication.

At the age of forty (1853), what with his larger and smaller books, his original researches, his lectures on medical jurisprudence at University College, and numerous popular lectures on scientific topics, Dr. Carpenter's life was unusually laborious and productive.

In 1856 he was appointed Registrar of the University of London, and for twenty-three years administered the onerous duties of that office in such a way as to contribute in no small degree to the success of the University, and above all to the maintenance of the high

character of its degrees and the ample recognition of the study of natural science for which the University is now distinguished.

He was able now to give a larger amount of time than formerly to his original investigations, and, in his summer holidays at Arran and elsewhere, commenced, amongst other studies, those researches on the structure and development of the beautiful little feather-star, which were from time to time published in the "Philosophical Transactions," and led to his association with Wyville Thomson, and thus to the deep-sea explorations of the "Lightning," and subsequently of the "Challenger."

Carpenter's memoirs on Comatula give a very full and beautifully illustrated account of the structure of the skeleton of the feather-star, but for many years the view which he entertained with regard to the nature of the axial cord which runs through the segments of the arm-skeleton of that animal was regarded by all other observers (with scarcely an exception) as erroneous. Dr. Carpenter considered these cords as nerve-cords, and in the Easter vacation of 1876 he made a special visit to the marine laboratory erected by Dr. Dohrn at Naples, in order to test his views by the repetition, on an extensive scale, of experiments which had already appeared convincing to his mind. These experiments, and others since carried out by younger naturalists, have at length fairly established the view for the truth of which the veteran observer had long contended.

In December, 1875, Dr. Carpenter had communicated to the Royal Society the outlines of his work on the soft parts of *Antedon* (*Comatula*) *rosacea*, and on returning from Naples he communicated a supplemental note to the Society on the subject of the nervous system of that animal ("Roy. Soc. Proc.," vol. 24, 1876, p. 451). He lived to see his conclusions, first formulated in 1865 ("Phil. Trans.," vol. 156, p. 705), fully confirmed by the experiments of Marshall ("Quart. Journ. Micro. Sci.," vol. 24, 1884) and by those of Jickeli of Jena ("Zool. Anzeiger," No. 170, 1884). Important evidence in favour of these conclusions was also furnished by the anatomical investigations of Dr. P. Herbert Carpenter on the allied genus Actinometra ("Journal of Anatomy and Physiology," 1876). M. Ed. Perrier of Paris, who had strenuously opposed Dr. Carpenter's conclusions, was thus led to renewed observations and to a complete acceptance of their correctness ("Comptes Rendus," July, 1883, p. 187), whilst the most careful student of Echinoderm anatomy among German zoologists, viz., Dr. Ludwig, who had also previously opposed these conclusions, has now endorsed them. Thus in the "Roy. Soc. Proc.," vol. 37, 1884, p. 67, Dr. Carpenter was able to give a complete history of the question, showing how the opposition on theoretical grounds to the view that the axial cords of Comatula were nerve-cords, had gradually given way before an appeal to observation and experiment. This

record of the final triumph of the view which he had originated and so long laboured to establish was the last scientific paper published by Dr. Carpenter.

The deep-sea explorations which Dr. Carpenter, assisted by Professor Wyville Thomson, arranged, and for which he succeeded in obtaining the aid of ships of the Royal Navy, were designed not merely to search for organisms in the great depths of the ocean, but especially to study the ocean currents both deep and superficial, Dr. Carpenter having a strong desire to enter upon the explanation of the great physical phenomena presented by the ocean. He himself took part in the earlier expeditions in 1868 and subsequent years, and though unable to leave the ties which bound him to home, so as to join the "Challenger" Expedition, yet he closely watched the results then obtained, and embodied the whole of his observations, and those reported from the "Challenger," in some extremely suggestive and important memoirs and lectures on ocean circulation. These are as follows:—Reports in the "Proceedings of the Royal Society" on the several cruises of the "Lightning" ("Roy. Soc. Proc.," 1868), "Porcupine" (*ibid.*, 1869–70), "Shearwater" (*ibid.*, 1871), and "Valorous" (*ibid.*, 1875); a series of memoirs in the "Roy. Geograph. Soc. Proc.," 1871, 1874, 1875, 1877; lectures delivered to and printed by the Royal Institution of London and the United Service Institution; articles in the "Contemporary" and the "Nineteenth Century" Reviews.

The more general philosophical views held by Dr. Carpenter, and his conceptions in regard to those topics where science touches religion, may be gathered from a series of articles published by him in the "Modern Review" during the years 1880–84, the titles of which are as follows:—"The Force behind Nature," "Nature and Law," "Charles Darwin; his Life and Work," "The Doctrine of Evolution and its Relations to Theism," "The Argument from Design in the Organic World."

In 1879 he retired from the Registrarship of the University of London with a well-earned pension, and was at once chosen as a member of the Senate of that body. He now devoted himself with unabated vigour to the prosecution of his studies on Foraminifera and on Comatula, and to more theoretical matters, such as ocean-currents, and the explanation of the frauds of spirit-mediums. Though released from the duties of office, he was still a constant attendant at the Senate of the University, he rarely missed a meeting of the Royal Society or one of the annual gatherings of the British Association, and, besides undertaking the administration of the Gilchrist Trust, delivered many lectures in all parts of the country himself—both independently and as an emissary of the trustees. The scheme of lectures and scholarships instituted by the Gilchrist trustees, which

is effecting important educational results in natural science among classes of society excluded from regular University teaching, is Dr. Carpenter's work. He wrote at this time in the interest of the public health some admirable articles on vaccination, as in earlier life (1849) he had from a similar point of view treated the subject of alcoholic liquors, and had urged the arguments for total abstinence. When past seventy years of age he did not shrink from a journey to the United States, where he spoke and lectured with unflagging vigour. The last public movement in which he took an active part was the foundation of the Marine Biological Association, of which he was a Vice-President, and which is about to carry out, by means of its laboratory on Plymouth Sound, a suggestion which is traceable to his own proposition for the thorough exploration and study of Milford Haven.

The abundant and noble achievements of Dr. Carpenter's public and scientific career did not pass without recognition in the form of awards and titles. He received in 1861 one of the Royal medals awarded by the Council of the Royal Society, and in 1883 the Lyell medal of the Geological Society. In 1871 he was made an honorary LL.D. of the University of Edinburgh, and in 1872 he was President of the British Association for the Advancement of Science when it met at Brighton. In 1873 he was elected Corresponding Member of the Institute of France, and on his retirement from his official position at the University of London in 1879 he was nominated C.B.

It is impossible to do justice to Dr. Carpenter's character as a scientific man in a few lines: here no attempt has been made to do more than indicate in something like chronological order and connexion of subjects the vast amount of work which he accomplished.

Upon the present writer, whose father was his fellow-student at University College, and who has enjoyed since boyhood the privilege of his friendship, Dr. Carpenter always produced the most vivid impression of a man of indomitable energy, who had accepted as the highest duty and keenest delight of his life the promotion, whether by advocacy or by research, of true knowledge. The tenacity and vigour with which he was wont to expound his views on such matters of research as at the time occupied his thoughts, and the importance and respect which he assigned to all genuine research, were evidences of an earnest and just nature which evoked sympathy and esteem in all men of kindred pursuits.

In reference to Dr. Carpenter's private life and tastes, the following extract from a weekly journal states, with the authority of a member of his own family, what might, in its absence, have been here less completely indicated. The journal to which we are thus indebted is an organ of the Unitarian Church, of which body Dr. Carpenter was, throughout life, an active and orthodox member, a fact which may or

may not be brought into connexion with the fact of his incomplete acceptance of the leading doctrines of Darwinism.

"He was well versed in literature, and turned for refreshment in hours of weariness to his favourite Scott. Political memoirs of his own time were read with the keenest relish, for he had early learned from his father, Dr. Lant Carpenter, to take a high view of a citizen's obligations, and the Bristol riots, which he had witnessed, made a life-long impression upon him. A brief sojourn in Italy called forth a susceptibility to the enjoyment of art which was a surprise even to himself; and in music, from the time that he had taught himself as a young man to play on the organ, he found unfailing recreation. Nature, likewise, in her vaster as well as her microscopic forms, was for him full of charm and delight, and from every excursion he carried back memories which remained singularly vivid and distinct. In society his immense stores of information, his sympathetic interest in others, his thorough enjoyment of humour though he felt unable to originate it, made him a genial and ever-welcome companion, while his friends learned how strong a confidence might be placed in his faithfulness. Many young men found unexpected help and encouragement in him, and he rejoiced when he could open a way to those who were involved in the struggles through which he had himself once passed. The dominant conception of his life—as was fitting in one of Puritan descent—was that of duty. And if this sometimes took austere forms, and led him to frame expectations which others could not always satisfy, an enlarging experience mellowed his judgment and enabled him to apprehend their position from their point as well as his own. Released from the pressure and strain of earlier life, he was able to give freer play to his rich affections; and in his own family they only know what they have lost who will never again on earth feel his support as husband and father, brother and friend."

E. R. L.

William Willoughby Cole, third Earl of Enniskillen, was born on the 25th of January, 1807, succeeded to the title and estates in March, 1840, and died at Florence Court, co. Fermanagh, on Friday, the 12th of November, 1886, after only a few days' illness. He sat as Baron Grinstead in the House of Lords.

Lord Enniskillen was educated at Harrow and at Christ Church, Oxford, but never graduated. While at Oxford he devoted much time and attention to Geology and Palæontology, assiduously attending the lectures delivered by the Rev. Dr. Buckland.

In conjunction with the late Sir Philip de Malpas Grey Egerton, his friend and college companion, Lord Enniskillen commenced collecting organic remains in the neighbourhood of Oxford and other parts of the British Islands; together they ultimately succeeded

in obtaining a most extensive, complete and valuable collection of Fossil Fishes; probably the largest in the world. From that period until the death of Sir Philip Egerton they were joint collectors, scrupulously dividing their acquisitions, and that so industriously, that nearly the same species occurred in each cabinet; it was owing to this union of partnership that the two collections were so intimately interwoven, and for the interest of Ichthyic Palæontology inseparable; hence their subsequent possession by the nation, and united preservation in the galleries of the British Museum of Natural History, Cromwell Road.

Three years previous to Lord Enniskillen's death (1883), that portion contained in the Florence Court Collection was purchased by the trustees of the British Museum, through a special grant from the Treasury.

Lord Enniskillen* and Sir Philip Egerton, after leaving Oxford, travelled through much of Germany, Switzerland, and Italy, solely for the purpose of studying and collecting one group of the Vertebrata, and to this large division of the animal kingdom they appear always to have restricted their researches; their continental journey was also undertaken to still more perfect their knowledge of stratigraphical Geology, in connexion with their palæontological researches; how completely this was continued, and carried on until both our distinguished Fellows were removed by death, is fully exemplified by the union of the two great collections—or that of Florence Court and that of Oulton Park—now arranged together in the galleries of the British Museum of Natural History.

Lord Enniskillen also particularly examined the caves of Germany, Belgium, &c., obtaining from Gailenreuth, Kuhloch, and Engis, a rich series of bones illustrating the remains of extinct Mammalia, especially those of the lion, mammoth, rhinoceros, hyæna, and reindeer.

At home and abroad every locality and geological horizon yielding the remains of fish attracted Lord Enniskillen's attention, who never failed to secure every good as well as new form for his Florence Court Collection, duplicates whenever they occurred being also added to the Oulton Park Museum by Sir Philip Egerton.

This dual possession and study of Fossil Ichthyology arose through their intimate acquaintance in 1830 and subsequent years with Agassiz, who impressed upon them the importance of confining themselves to one line or branch of research; the result of which has been the formation of these two unrivalled collections of Fossil Fish, the history of which, through the works of Agassiz and Sir Philip Egerton, has greatly enriched the literature of this extensive division in Zoology, especially as regards structure and distribution.

* Then Viscount Cole.

Lord Enniskillen never published any particulars of, or described any species of Fossil Fishes, but the Florence Court MS. catalogue was kept with the most scrupulous care, every specimen being recorded with all details essential to the zoological position, stratigraphical and geographical history of the species in so great a collection; and this was carried out in every detail.

This applied not only to the Florence Court catalogue, but also to the almost *duplicate* volume of the Oulton Park Collection, kept with the same care by Sir Philip Egerton; so that the enumeration of species contained in one, without reference to the other, would be incomplete and unsatisfactory.

Lord Enniskillen paid considerable attention to Archæology, especially that relating to Ireland, and was one of the first to call attention to the lake dwellings in that country.

Lord Enniskillen's public services were great, as also indeed had been those of the long line of his ancestors through nine generations, ever since their settlement in Ireland in the year 1612. He sat as M.P. for Fermanagh from the year 1831 to 1840; was Colonel of the Fermanagh Militia from 1834 to 1875, and Hon. Colonel from 1875, and for more than fifty years leading member of the Orange Society, Grand Master of the County of Fermanagh, Grand Master of Ireland, and Imperial Grand Master. He was also one of the Trustees of the Hunterian Museum. He received the honorary degree of D.C.L. from the University of Oxford, and that of LL.D. from the University of Dublin and the University of Durham.

Lord Enniskillen was elected F.R.S. in the year 1829, and was thus a Fellow of our Society for fifty-seven years.

R. E.

Thomas Andrews was born at Belfast on the 19th December, 1813. His father was a linen merchant in good position. He received his early education at the Belfast Academy and at the Royal Academical Institution of Belfast. He then went to Glasgow to study chemistry under Professor Thomas Thomson, whose laboratory was then one of the very few places in this country where systematic instruction in *real* chemistry was regularly given to students. He continued his studies in Trinity College, Dublin, where he distinguished himself both in Science and in Classics; and, after spending some time in Dumas' laboratory in Paris, went to Edinburgh, where he took the degree of Doctor of Medicine in 1835. Returning to Belfast, he devoted himself to the practice of medicine, in which he was very successful. In 1845 he was the first Lecturer on Chemistry in the Royal Belfast Academical Institution, but held this office for a short time only. In 1845 the Queen's Colleges were founded and Andrews was appointed Vice-President of the Belfast College. With this office

there was conjoined, when the preliminary arrangements had been made, that of the Chair of Chemistry. He held these offices till 1879, when the state of his health induced him to resign them, and to retire almost completely from active work. He continued to take a keen interest in the progress of Science till his death on the 5th November, 1885.

He was elected a Fellow of the Royal Society in 1849, he was an Honorary Fellow of the Royal Society of Edinburgh, and a Corresponding Member of the Royal Society of Göttingen. He received honorary degrees from various Universities.

He presided over the Chemical Section of the British Association at Belfast in 1852, and again at Edinburgh in 1871, and was President of the Association at the Glasgow Meeting in 1876.

In 1842 Dr. Andrews married Jane Hardie, daughter of Major Walker of the 42nd Highlanders. He is survived by Mrs. Andrews, by three daughters and by two sons, the elder of whom is Major in the Devonshire Regiment, and the younger a member of the Irish Bar.

Dr. Andrews was deeply interested in public affairs, but very rarely took an active part in politics, and was quite free from party spirit. His only writings bearing in any way on political matters are 'Chapters of Contemporary History.' The first, entitled "Studium Generale," and published in 1867, is a historical and critical discussion of the function of a University, with special reference to the Queen's Colleges. The second, "The Church in Ireland," was published in 1869.

Of Dr. Andrews' strictly Chemical papers we may mention one on the blood of cholera patients, in which he showed that it differs from normal blood only by having a smaller proportion of water; one on galvanic cells with strong sulphuric acid as the exciting liquid, and one on the presence of metallic iron in basaltic and other rocks. Much more important than these careful and interesting papers is his great work on Ozone. This mysterious body had been the subject of investigations by its discoverer, Schönbein, and also by Marignac, De la Rive, Berzelius, Williamson, Fremy and Becquerel and Baumert, but its real nature was still unknown, it was not even certain that a number of different substances had not been confounded under the name. That ozone, however prepared, contained oxygen, and was a powerful oxidising agent was certain, but it was not clear that it did not, sometimes at least, contain hydrogen also. Andrews attacked the problem with characteristic energy and straightforwardness. By a series of experiments, in which it is difficult to say whether the ingenuity, the perfect fitness for their purpose, or the wonderful simplicity of the methods used is to be most admired, he proved that "ozone, from whatever source derived, is one and the same body, having identical

properties and the same constitution, and is not a compound body, but oxygen in an altered or allotropic condition."

This work was continued by Andrews and Tait, and the results were published in the 'Philosophical Transactions' under the title "On the Volumetric Relations of Ozone and the Action of the Electrical Discharge on Oxygen and other Gases." The theory of the constitution of ozone now universally held is clearly indicated in this paper, although its apparent improbability deterred the authors from discussing it fully.

But by far the most interesting and peculiar part of Dr. Andrews' work is to be found in his investigations in the borderland between Chemistry and Physics. There his special ability, his power of arranging experiments, of devising pieces of apparatus suited to the particular purpose at the moment in view, of detecting sources of error and providing simple effective means of avoiding them, and of doing all this himself with the least possible help from the instrument maker, comes into remarkable prominence. The apparatus with which most of his work was done was made with his own hands, and when, for instance, he wanted a casting, he personally superintended the minutest details. This directness of his work, and his habit of working alone, made him somewhat intolerant of assistance, as it made him independent of it. The only exception to the solitariness of his scientific work, a very notable exception, is the investigation of Ozone, carried out in conjunction with Professor Tait. His researches on the heat developed in chemical actions, for one of which he received in 1844 one of the Royal Medals, and for the other in 1850 one of the prizes given by the French Academy of Sciences, and that on the continuity of the liquid and gaseous states, partly contained in his Bakerian Lectures, and partly communicated posthumously to the Society, were done strictly alone. If this independence and individuality limited the amount of work done by him, it has the compensating advantage that we know that every analysis and every observation published by him were actually made with his own hands and eyes, so that a reader of his paper is as nearly as possible in as good a position to judge as to the soundness of his conclusions as if he had performed the experiments himself. In reading his papers we are transported at once to the laboratory; without wearisome repetition we have all the details before us, and we can follow every step of his argument as if we had been present at every experiment on which it was founded.

The investigation into the heat given out during chemical action was begun while he was still engaged in medical practice; this work at once established his position as a genuine scientific discoverer, and introduced him to the chemists and physicists of Europe.

But the work which will always be most closely connected with his name is the great investigation into the relation of temperature, pres-

sure and volume of carbonic acid, communicated to this Society in the Bakerian Lectures of 1869 and 1876.

He showed that for temperatures below about 30·9° C. as the pressure is increased we come to a point where condensation occurs, where the gas is converted into a liquid. At this pressure there are two limiting values of the volume, one when all the substance is vapour, and one when it is all liquid. Between these the substance is partly vapour and partly liquid, the distinction between the two being visible, as the liquid and the vapour refract light differently, and are separated from one another by a distinct meniscus. As the temperature approaches 30·9° the abrupt change of volume on condensation becomes less and less in amount, above that temperature (the *Critical Temperature* of carbonic acid) there is no abrupt change of volume, and no visible condensation. Near 30·9°, but above it there is a rapid, but not abrupt, change of volume; as the temperature rises this rapid change of volume becomes less and less marked, and the isothermal approximates more and more to the hyperbolic form. This extraordinary character of the isothermals—discontinuous below a particular temperature, continuous above it—led Andrews to the remarkable discovery which gives the title to his Lectures, "The Continuity of the Gaseous and Liquid States of Matter."

If we represent the relation of temperature, pressure and volume by means of a surface, where the rectangular co-ordinates x, y, and z correspond to p, t, and v (as was done by Professor James Thomson to illustrate Andrews' work), we see that there is what may be called a cliff, points at the top of which correspond to the substance at the condensing point, but all in the state of vapour, while points immediately below them, at the base of the cliff, correspond to the substance just condensed and all in the state of liquid. The cliff becomes less and less high as x and y increase, and vanishes at the place corresponding to the critical point. From a point on the slope above the cliff we can pass to a point on the slope below, either by dropping down the vertical face or by going round the end of the cliff. To quote Andrews' words " . . . . the author has made carbonic acid pass, without breach of continuity, from what is universally regarded as the gaseous to what is, in like manner, universally regarded as the liquid state. As a direct result of his experiments, he concludes that the gaseous and liquid states are only widely separated forms of the same condition of matter, and may be made to pass into one another by a series of gradations so gentle that the passage shall nowhere present any interruption or breach of continuity. From carbonic acid as a perfect gas to carbonic acid as a perfect liquid, the transition may be accomplished by a continuous process, and the gas and liquid are only distant stages of a long series of continuous physical changes. Under certain conditions of temperature and pressure, carbonic acid finds

itself, it is true, in a state of instability, and passes, without change of pressure or temperature, but with evolution of heat, to a condition which, by the continuous process, can only be reached by a long and circuitous route."

Like most great discoveries, this had been to a certain extent foreshadowed. In 1822 Cagniard de la Tour observed that certain liquids —ether, alcohol, water—when heated in hermetically closed tubes, were apparently totally changed into vapour occupying from two to four times the original volume of the liquid. In 1823 Faraday succeeded in condensing to liquids a number of substances previously known in the gaseous state only.

Shortly afterwards Thilorier obtained solid carbonic acid, and observed the very rapid expansion of liquid carbonic acid when heated. In 1845 Faraday published a very remarkable paper in the 'Philosophical Transactions' on the liquefaction and solidification of gases. He there pointed out that as different liquids assume the Cagniard de la Tour state at different temperatures, so the gases which had not been condensed—oxygen, hydrogen, nitrogen, &c.—might be supposed to have that point below the lowest temperature he had applied (that of a bath of solid carbonic acid and ether *in vacuo*), and therefore to be incapable of condensation to a liquid by any pressure unless the temperature were much further lowered.

Faraday conjectured from the results of experiment that the Cagniard de la Tour state occurred in the case of carbonic acid about 90° F., a value surprisingly near that experimentally proved by Andrews.

But what others had seen obscurely or partially, or had inferred, Andrews made clearly visible in its entirety, and many physicists and chemists can testify to the startling character of the revelation made by the publication of his discovery. It is in fact after the discovery has really been made that the historian begins to look for foreshadowings, and we are perhaps somewhat inclined to interpret these early indications in the light of the later, more definite knowledge, but the unanimous verdict of the scientific world is that the discovery of the continuity of the liquid and gaseous states belongs to Andrews and to him alone.

Dr. Andrews was loved by all who knew him. Warmly hospitable, he was personally almost stoically temperate, allowing himself the minimum of rest, of food, and of sleep. While in theological, as in all other matters, he thought for himself, he was a consistent and orthodox Christian and a loyal member of the Church of Ireland.

A. C. B.

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